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LECTURES
ON
COMPARATIVE ANATOMY
AND
ANIMAL PHYSIOLOGY,

DELIVERED BY

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LECTURE XXXVI.

ON THE NERVOUS SYSTEM OF THE RADIATED AND HELMINTHOID CLASSES.

THE nervous system communicates to the muscles their energy of action, and by its extensive distribution and its sensitive properties it establishes an instantaneous communication between all parts of the body—indeed it is by this system that animals are connected with outward nature, and there is no part of their economy the condition of which is more indicative than this of the condition of the whole, and of the grade which an animal occupies in the scale. The nervous system is present in every division of the animal kingdom, and has been detected in almost every class. It is everywhere connected with sensation and motion, and its general form corresponds with the form of the body, being short and circular in the short round bodies of the radiated and molluscous animals, and having an extended form in the lengthened trunks of the articulated animals and the vertebrata. It consists of white-coloured opaque globular particles, enclosed in very minute tubular sheaths, formed of condensed cellular tissue; and these extremely minute tubular filaments, filled with soft globules, unite into fasciculi, without anywhere anastomosing, and they are developed from the periphery of the body towards the centre. These filaments appear to terminate in the cineritious substance of ganglia, or to return from that substance with different properties, and they compose by their aggregations the nerves of the body, and the whole spino-cerebral axis of the vertebrata. We perceive great regularity and a unity of plan in the development of this system, whether we examine its successive stages of formation in the same individual at different periods of life, or trace the phases of its development in the great body of the animal kingdom.

Almost all the known genera of *polygastric* animalcules possess eyes, generally of a reddish colour, sometimes two, united together on the median plane, like a *monoculus*, most frequently the two separate from each other, and cases of monstrosity in these animals have been seen with three. They are seen even in the monads, and I have shown long since that the *cercariæ* are highly sensible to light, like hydræ, medusæ, actiniæ, and other supposed nerveless animals. These animals are found to possess an acute sense of taste, they distinguish, pursue, and seize their prey, they avoid impinging on each other while swimming crowded in a drop of water, they contract and bend their body in every direction, they increase, or retard, or cease at pleasure, their progressive motion and the vibration of their cilia, like the gangliated and muscular rotiferous animalcules. Yet a nervous filament has not been detected in the transparent microscopic bodies of the polygastrica. All their movements appear to be as regular, methodical, spontaneous, and well-directed, as those of many higher classes of swimming animals. They never appear like particles of dust, driven to and fro in the water by currents or other mechanical agency, and they will probably be found to have a nervous system, a rudiment of that of the rotifera, and form the root of the helmin-

thoid division of the animal kingdom. The *cercariæ virides* I have found to be, like the *hydra*, fond of light, and always to accumulate on the lighted side of a vessel of water as often as the position of the vessel is changed, and they are often found accumulated as a green film on the south side of our pools of fresh water in summer, the side next to the light at noon. The possession of a single eye on the median plane in many of these *cercariæ* and other polygastric animalcules, and in many of the entomostracous crustaceous animals, composed of the half of the right eye joined to the half of the left, shows that these simple animals present as a normal character of this organ, what we frequently find as a monstrosity in the higher animals and in man.

In the soft gelatinous bodies of the *poriferous* animals, composed of simple cellular tissue, no nervous filaments have ever been detected, nor is it probable, from analogy, or from the general structure and living phenomena of these animals, that they possess a nervous system as a centre of reference for all external impressions, or to direct those imperceptible movements on all points of the body, by which the currents are conveyed inwards through the superficial pores. Their nervous globules, like the muscular, are diffused through every part of the homogeneous cellular tissue of the body, which possesses the same properties in every part, and is indefinitely divisible without destroying its vitality. Their ciliated gemmules, however, are endowed with remarkable living properties, and powers of spontaneous motion. They have an obvious object in their motions, they can accelerate, retard, or cease, at pleasure, the vibrations of their cilia, they can change the direction of their course in the water, perceive each other's vicinity, revolve round each other, distinguish the most suitable place for the fixing of each species, or bound forward suddenly from a state of rest. A single gemmule vibrates more than a thousand visible cilia, and with as much precision and effect as a vibrio or a vorticella; yet these gemmules contain no visible fibre, and are composed of the same soft granular or cellular matter as the adult animal, and they already present several spiculæ developed in their interior. They appear in this state of freedom and activity to be sensible to light and to shun it, as I have found them to accumulate, fix, and grow, most generally on the darkest or most shaded part of the vessel in which they were placed. Indeed there can be little doubt that the gemmules of poriferous animals and of zoophytes are principally guided by the influence or intensity of light to the most suitable place for the fixing and growth of each species.

The *actinia* resembles a large detached polypus of a *caryophyllia*, or of a *meandrina*, and it is generally considered to be like the *hydra*, a detached polypus of a zoophyte, and placed among the *polypiferous* animals. Its nerves were described and figured by Spix nearly thirty years ago. They consist of filaments which surround the base of the foot, and having minute ganglia in their course, from which nerves pass out chiefly to the circumference and to the muscular parts, which here possess great power of contraction. I have caused the common *actinia crassicornis* to lift a basin of sea-water, weighing more than six pounds, by making it swallow forcibly the perforated shell of a *purpura lapillus*, through the perforation of which a chord was passed. By the chord the basin was lifted without the actinia quitting its hold, either of the shell in its contracted stomach, or the basin attached to its muscular and highly sensible foot. The sphincter muscle around the upper part of this animal is of great size and power, to prevent the escape of its prey when seized. Its perpendicular muscles are also of great strength, disposed as vertical partitions between the ovaries. I have found the actiniæ to be sensible to light, and to shun it, collecting and resting on the darkest and most shaded part of vessels of water in which they were kept. They generally seek dark and concealed places in the sea, and these signs of aversion, or of attachment to light in a state of confinement, generally correspond with the living habits of the species in the poriferous and polypiferous animals. The *hydra*, like the *cercariæ*, seeks the light, and is, therefore, generally found attached to the little stalks of the *lemnæ*, which float with their expanded leaves on the surface of stagnant pools. By this position it is best able to hang down and move with freedom its long tentacula in search of little annelides swimming through the water. The polypus of a *fungia agariciformis* is single, and rises from the central cavity, like a large actinia, the polypus of the *caryophyllia cyathus* is also single, and like an actinia which has formed for itself a calcareous cell. These large isolated polypi, with isolated cells, resemble the actiniæ very closely in form, structure, and habits, and have probably the rudiments of the same form of nervous system. But these isolated polypi agree most closely in size, form, and structure, with those of the ramified *caryophyllia*, the ponderous *meandrinæ*, and many other large calcareous

compound zoophytes, so that it is not probable that a system so important and general in its influence could exist in one of all these forms, and not in another so closely allied in every respect.

In the *acalepha* we perceive animals of great size, swimming freely through the sea by their own exertions, possessing a complicated digestive apparatus, extending through the body, and having for the most part numerous long tentacula, exquisitely sensitive and contractile. They occur associated in great numbers, they seize and digest living prey, and they appear to be sensible to light, which they shun, by directing their course downwards when they have been affected by it. From the softness and transparency of their bodies, the light may penetrate and pass through every part of their delicate texture, and affect them with sensations of touch, though not with visual impressions. In the larger *medusæ* the pulsations of the mantle are slow and regular, and in these no nervous system has yet been perceived or described. The *physalia* quickly depresses its crest and escapes when alarmed. In the minute *equoreæ*, the movements are most lively, the contractions of the mantle are quick, strong, and irregular, and the tentacula which bind its margin are highly sensitive and contractile; their gemmules I have found to be ciliated on the surface, and to move spontaneously like those of zoophytes. The tentacula of *beroe* are also exquisitely sensible and irritable, and this animal can move its large fin-like cilia, with iridescent hues, in different directions and with different degrees of velocity, at pleasure. It can dart out or retract its tentacula with great quickness, extend or retract its oral lips, and move its body in every direction, and with either extremity forward. Around the oral extremity of the body in the *beroe pileus* (*Fig.* 123, page , Vol. II), is seen a double circular nervous filament, which sends off minuter filaments in each of the spaces between the eight longitudinal bands of cilia. These eight points from which the longitudinal filaments come off present minute ganglionic enlargements, like those seen on the circular oral nervous chord of an asterias.

In the *echinoderma* the cellular tissue is more consistent, the muscular fibres are more distinct, and nervous filaments have been long known to exist in several of the species of this class. The asterias, like the actinia, has but one opening into the alimentary canal, placed in the centre of the lower part of the body. Around this stomach, which has but a single aperture, a distinct nervous chord is seen, white and opaque, which forms a circle around the entrance to this digestive cavity. At regular distances there are seen around this nervous circle minute enlargements or ganglia, from which nervous filaments extend to the sides of the stomach and to the divisions of the body. Those radiating nervous filaments passing to the rays of the *asterias*, are observed to pass one from each ganglion in a straight line along the middle division of each part of the body, which in the diagram before you is supposed to be divided only into five rays, as it is in most of the species. Sometimes there are as many as sixteen divisions or more of the body in the common *asterias papposa*, in which case there are as many nervous filaments given out from around the oral nervous ring. But besides those five distinct filaments sent, one from each ganglion, to the five rays, other two nervous filaments are observed to pass from each of those ganglia, which filaments are confined to the central part of the body, and do not pass on to the divisions or rays of the body.

This circular disposition of the nervous filaments seen in the acalepha and the echinoderma, seems to be the lowest form of that system met with in the animal kingdom,—a simple circle of nervous filaments around the entrance of the alimentary canal. This first or lowest condition of the nervous system, with its central part, placed near the mouth so as to watch over the kind of substances that are conveyed into the alimentary canal, we can trace with certain modifications through all the succeeding forms of the nervous system, to that of man himself. We see this character of the nervous system throughout the articulated and molluscous classes of animals; thus the accumulation and concentration of the nervous system are chiefly at the anterior part of the body, upon or around the œsophagus. We see that that supra-œsophageal part of the nervous system in the vertebrated animals is enormously developed, and in all animals where there are numerous complicated and large organs of the senses, it is placed in the vicinity of the entrance of the alimentary canal, to attain a knowledge of all the properties of the exterior substances which surround them, that they may be able to distinguish what is hurtful from what is suitable to their nature. Thus it is that we observe in these lowest animals that those few nerves are not provided for some particular organ of sense, but appear first to be developed around the alimentary canal as the most important part to be provided with sensibility; the most important part to take cognizance of the properties of surrounding bodies, and thus to have more in-

timate relations established with surrounding nature. An animal is, in the abstract, but a moving bag, which receives foreign matter into it, and which is organized to change that matter into its own likeness. That is the most unsophisticated idea which we can form of this great division of nature. But we see how complex the structure at length becomes by the development of other systems of organs from the cutaneous and the digestive, and by the numerous complicated organs of sense which are developed in that situation to superintend the nourishment of the whole, and to multiply and extend the relations of animals with external nature. This circular form of the nervous system around the mouth of the *stellerida*, is no doubt extended to the more elevated forms of the *echinida*, where the muscles, especially of the jaws or teeth, are so distinct and powerful. This circular form of the nervous system corresponds with that of the body, and is equally developed around a short axis, which is generally vertical, with the mouth downwards towards where the food lies. It is from this common character of the nervous system, when it is present in these lowest classes, that this division of the animal kingdom is called *cyclo-neura*.

Now in the *holothuria*, — which is an animal also belonging to the class echinoderma, though possessing a soft and irritable exterior integument, and possessing very distinct strong longitudinal muscular bands extending along the inner surface of the skin,—we find that, accompanying this highly developed condition of the muscular apparatus, there is a very distinct and complex nervous system. That nervous system still exists in these lengthened animals in the form of a collar around the anterior part of the body, giving off longitudinal filaments. We observe, also, longitudinal nerves passing from the anterior nervous ring along one surface of the body, in the *siponculus*; it appears thus to introduce a difference on the surface, a dorsal and a ventral aspect, in this vermiform echinodermatous animal. Thus in these lower tribes of echinoderma and acalepha, we observe the parts of the body and the nervous system equally developed around an axis, so that there is no accumulation of the nervous matter upon one side of the body more than upon another; nor is there the digestive or any other system preponderating on one side to produce the distinctions of dorsal and ventral surface. But in these soft vermiform echinodermatous animals, the holothurida, which are echini, lengthened, and without shells, we find that one surface has assumed the character of abdominal, in consequence of the greater longitudinal development of the nervous system along that side of the body. This gradually leads us, therefore, as well as the lengthened cylindrical form of the body, and the soft flexible integument, to the long articulated forms of higher classes, and especially of the helminthoid animals.

In the long cylindrical trunks of the helminthoid and entomoid classes of animals, the nervous system partakes of the same lengthened form, and is extended for protection along the abdominal or under surface of the body. It is contained in the same rings or segments which envelop the viscera in all the vermiform classes, but in the highest entomoid classes it begins to be enclosed in an osseous canal separate from that containing the other organs. And in all these classes we have seen, that the motor and sensitive columns of these abdominal nerves are inverted in their position, like all the other viscera of the trunk, when compared with their position in the vertebrated classes. In the lowest of the helminthoid classes, the *entozoa*, the nervous system is seen in the long cylindrical bodies of the nematoid species, as the *ascaris lumbricoidis*, in the form of a slender double filament, occupying the median line of the abdomen, and placed immediately within the inner muscular tunic of longitudinal fibres. This abdominal chord appears as if single, but shows its double structure chiefly in two points, where it meets with the vulva of the female, and separates to enclose that opening, and at the lower part of the mouth, where it separates to encompass the œsophagus. These nerves are seen more obviously on the large *strongyli* and *echinorhynchi*, and many similar forms of entozoa. They generally ascend on each side of the œsophagus, so as to embrace it more or less completely with a nervous collar, and sometimes form a complete ring by meeting above the mouth. The surface along which they run is considered as analogous to the ventral surface of ordinary external worms, because the vulva and the penis and the anus are directed to that surface of the body. On looking through the transparent body of these simple entozoa, a fine white opaque streak is seen upon the ventral surface of the worm, extending towards the posterior part of the body. It is almost impossible to trace it the length of the anus, but it is reinforced by almost invisible filaments, and becomes stronger and more apparent as we approach the middle and thicker part of the body. When we trace it towards the opening of the genital system in the female, the vulva, it distinctly separates, and forms a circle

around that passage; the two filaments again meet and pass forwards, still upon the same surface of the body, and are sometimes so small as to be almost lost before they reach the head. At the anterior part of this double nervous filament, it is observed most frequently to bifurcate when it reaches the anterior part of the body, and to advance upon the sides of the œsophagus, as if it would encompass that passage and form a circle around it.

Now nervous filaments, like muscular fibres, are each distinct, though minute instruments, and are not like bloodvessels that anastomose, and ramify, and shoot out capillaries in all directions. No single or ultimate nervous filament in animals is seen to ramify, either in the highest or in the lowest classes.

Now this first condition of the nervous system, and the simplest forms it presents in the articulated classes, are interesting, when we consider it in relation to the first condition perceived of the nervous system of man and the vertebrated animals, for there we observe the nervous system to consist at first of a simple streak, a streak which is manifested on the outer surface of the germinal portion of the cicatricula. That streak is well known to be developed afterwards in the progress of the embryo, into the central part of the nervous system; it is the rudiment, in fact, of the two lateral columns of the spinal marrow. The progressive development shows that primitive streak to separate at its anterior part, forming there the first outline of the medulla oblongata. It again diverges, after converging a little, and forms the first outline of the optic lobes. It diverges again and encloses another space, which forms the minute hemispheres of the brain. But in the first stage of this development in the vertebrata, it presents the form and condition of the nervous system in these simplest of helminthoid animals, which themselves represent the embryo or larva state of the higher entomoid classes. In the epizoa which adhere to the surface of aquatic animals, the nervous system is more complicated, and these animals already possess sometimes antennæ and eyes. They have generally the two longitudinal parallel nervous chords running separately and at some distance from each other, as you observe in the *acktheres percarum*, and several others, so beautifully and accurately figured in these plates of Nordmann published last year in Germany. From the want of organs of sense on the anterior extremity of the body in most of the entozoa, we observe the very imperfect development of the nervous system in that direction, and that although it occasionally forms a ring around the œsophagus, it seldom forms a visible supra-œsophageal ganglion where these filaments meet. These longitudinal nervous filaments still retain great simplicity of structure and development at their anterior terminations in the *lernææ* among the epizoa, although you already see minute eyes joined on the median plane, like those of a polygastric animalcule or a monoculus, and antennæ on their small head. Their muscular system is also distinct, both the longitudinal bands which move the trunk, and the numerous transverse bands passing from the sides to the intestine. These simple longitudinal separate filaments of nerves, without ganglia, running along the abdominal surface of the entozoa and epizoa, present us with the embryo state of that system in the catterpillar, and in all the entomoid classes, before their abdominal ganglia are developed.

In the *rotiferous* animals, however, where we find a very complex muscular apparatus developed at the anterior part of the body for the rapid movements of the large cilia, and for the lateral motions of the strong jaws, the nervous system is chiefly developed in that situation. Their abdominal filaments are seen through their transparent bodies united together as one nerve along the median line, and giving off minute lateral branches in their course forwards. Arrived at the anterior part of the body, these nerves ascend to embrace the œsophagus, and form numerous ganglia on that ring, like a molluscous animal with its circle of ganglia around the entrance to the alimentary canal. The lateral ganglia of this gangliated œsophageal ring of the rotiferous animals, send numerous filaments to the surrounding parts, but are smaller than the supra-œsophageal ganglia placed above on the median plane. In those figures of Ehrenberg you will observe four lateral ganglia besides the large superior ganglion, placed around the mouth of the *hydatina senta*, a minute rotiferous vorticella. Where the nervous system is perceptible in these minute wheel-animalcules, it presents this plan of structure, combining the ordinary abdominal filaments without apparent ganglia of the simplest helminthoid animals, with the gangliated circle around the œsophagus of a gasteropod or a floating pteropod. Many of the larger entozoa, as the *cœnurus* and the *cysticercus*, present no nervous system, from their simple organization requiring none; but in the rotiferous animals it is more generally concealed by the extreme minuteness of the whole body. The large round ganglia around the œsophagus of these animals are disposed within the

circle of round muscular lobes for the motions of the vibratile cilia, and are of a darker gray colour than the light yellowish-white muscles of the cilia and of the lateral jaws. The muscles of the jaws form a large round mass anterior and interior to the gangliated œsophageal ring.

In the *cirrhopods*, entomoid animals enclosed in shells, like those molluscous animals, we also observe the great centre of the nervous system disposed along the abdominal surface of the body. It there forms two longitudinal chords, which have ganglia regularly disposed, and which present a very imperfect development at their anterior extremity, where they form a ring around the œsophagus. In the cirrhopods there is a very imperfect development of the anterior parts at the entrance of the alimentary canal, for the obvious reason that these animals are shut up in a close multivalve shell, with their heads fixed at the bottom of that cavity, like a conchiferous or a tunicated animal. What use could they therefore have for a complex apparatus of nerves or organs of sense at their anterior part? No complex development of organs is there seen; but as the respiratory organs and the ciliated jointed extremities are developed from the sides of the posterior extremity of the animal, there the nervous abdominal filaments and ganglia are distinct and visible, and give out radiating nerves to these articulated members, from the regularly-disposed pairs of ganglia along the inferior surface of the post-abdominal portion of the trunk. These abdominal chords, after forming five pairs of ganglia at the bases of the articulated feet, advance to the mouth, form a ring around the œsophagus, meet above and form a supra-œsophageal ganglion, from which nerves pass forwards and laterally to the muscles and to the visceral sac. The nervous matter which is accumulated in the rotifera on the collar around the œsophagus, is here removed from that anterior position, where it is little required, and is employed to enlarge the ganglia of the posterior part of the column, to give energy to the parts developed from that region of the trunk. This diplo-neurose character of all the cirrhopods, whether pedunculated or sessile, separates them more than any other organs from the molluscous classes of animals, where the nervous development is at the opposite end of the column, and connects them with the articulated classes, especially the helminthoid forms, where the supra-œsophageal and all the anterior ganglia are least developed. As the nervous system of the rotiferous animals has some affinity, in its œsophageal circle of ganglia, to that of gasteropods and pteropods, so we observe in this very short gangliated sub-abdominal column of the cirrhopods, with only five pairs of ganglia, an affinity to the nervous system of the tunicata and conchifera, where we observe three principal pairs of ganglia disposed longitudinally beneath the intestine, and seldom a supra-œsophageal ganglion developed.

In the class *annelides*, as in that of entozoa, we find organizations possessing very different degrees of complexity, and although they still present, like the caterpillar, only the trunk of the perfect insect, they exhibit rudimentary developments from the sides of the segments, of various extent and magnitude, the preparations for the future legs and wings, which materially affect the condition of the nervous columns, and the development of the ganglia along their course. We have already seen that the trunk of the articulated classes is inverted, and surrounded by the cyclo-vertebral elements of the vertebrata. So that while the alimentary canal continues along the middle of the trunk, the great centre of the vascular system is above the intestine, on the dorsal region, and the motor and sensitive columns occupy an inverted position along the ventral side of the trunk. The yolk bag or umbilical vesicle enters, therefore, by the back, to reach the intestine in these classes, and not by the abdominal surface, as in the vertebrata. In the long slender forms of these red-blooded worms, as in the *naids* and many of the *nereids*, the nervous columns along the middle of the ventral surface of the trunk are nearly as simple as in the filiform entozoa, and scarcely present ganglionic enlargements in their course, or supra-œsophageal enlargements, from the imperfect development of their lateral appendices for progressive motion, and of their cephalic appendices for sensation. In the transparent bodies of the *planariæ* nervous filaments are scarcely perceptible. In the long cylindrical and muscular bodies of the air-breathing earth-worms, with their myriads of short segments, and almost without feet, we perceive innumerable distinct filaments passing laterally from the closely approximated nervous columns, but scarcely a ganglionic enlargement developed in their course along the abdomen. Arrived, however, below the mouth, they form two small ganglia of a lengthened form, and extend upwards to meet above the œsophagus, and develop the two larger cerebral or supra-œsophageal ganglia, the intimate structure and nerves of which you perceive minutely detailed in these large figures of Morren. These cerebral ganglia of the earth-worm lie in contact with

each other, are lengthened transversely like those of an insect, appear to be more gray-coloured on the surface than their white interior, and to be chiefly composed of minute globules. Although there are not distinct ganglia on the abdominal chords, these longitudinal columns are proportionally thick, to supply the innumerable muscles connected with the short and highly moveable segments. From the shortness of the segments, the lateral filaments are closely approximated, and as the columns are slightly enlarged at the coming off of each pair of nerves, the whole abdominal chords have a waved outline, from the number and proximity of these minute enlargements.

Where the trunk of the annelides is much developed transversely, as in the *leech* and the *halithea* or sea-mouse, the ganglia are much more developed and distinct. The abdominal ganglia of the leech are quite obvious in the embryo of that worm while it is yet in the ovum, as you observe in these magnified figures of its nerves given by WEBER. Where the setæ for progressive motion are large, and moved by strong extensor and retractor muscles, the ganglia are equally large and distinct, and the nervous columns greatly developed, as you perceive in these large *pleiones* from the Indian seas, and in most of the lubicolous annelides. By looking through the bodies of these common *pectinariæ*, as transparent as glass, you can distinctly perceive every ganglion, large, white, and opaque, along the abdominal surface, and the nerves which they successively give off. Indeed, from the uniformity in the situation and the distribution of the nervous filaments in all the articulated classes of animals, and their opaque white colour and firm consistence, there is no part in the economy of the most delicate species which is more easily examined. The most marked differences which this system presents in the helminthoid classes are the extent to which the nervous filaments are distributed through the body, the thickness of the two abdominal columns, the size and number of the ganglia on their course, the magnitude of the cerebral ganglia for the cephalic appendices, the relative magnitude of the visceral or sympathetic nerves beneath them, and of the ganglionic and motory columns. As each segment of the *tænia* is complete in itself, and capable of independent existence, so each segment of the body throughout the helminthoid and entomoid classes may be viewed as a separate being, with its exterior covering and its muscles, its vascular and digestive apparatus, its brain, and its nerves of motion and sensation. And hence the divisibility of the body in many of these animals, without destroying the vitality of the whole. The nervous œsophageal ring of the *actinia*, the *beroe*, and the *asterias*, we have seen continued through these helminthoid classes, but the lengthened form of the worm has given a preponderance to the development of the longitudinal filaments extending from the oral ring of the *siponculus* and the *holothuria*.

CLINICAL LECTURES

ON

DISEASES OF THE UTERUS AND ITS APPENDAGES;

Delivered at the HOPITAL LA PITIE, *Paris*, 1834.

BY M. LISFRANC.

ON AMPUTATION OF THE NECK OF THE UTERUS.

1.—*General Considerations.*

THE period, unfortunately, is not very distant from the present time when diseases of the uterus were involved in great obscurity. On the one hand they were considered as essentially fatal, when they had made a little progress; on the other they were completely mistaken or neglected, as inconveniences too simple to arrest the attention of the surgeon. In the former case the practitioner, struck with the inefficacy of his art, found himself compelled to employ nothing but palliative measures, and saw his patient fall a victim to the most violent suffering. But within a few years numerous researches made in pathological and surgical anatomy have thrown considerable light on this important part of pathology. The means of examination have become more certain and numerous, and the causes of uterine affections have been traced with a care and attention which have not failed to produce the most happy results. It has been shown, for example, and experience sanctions the assertion, that muco-purulent discharges, uterine hemorrhage, and prolapsus of the uterus, were not essential diseases, as had been imagined at the time, but symptoms of an alteration more or less grave of the tissues of the organ.

By a more rigorous examination of the symptoms revealing the alterations of the uterine tissues, we are now enabled to establish new distinctions to prove, by pathological anatomy, and the effect of treatment, that many uterine diseases are

much less dangerous than have been imagined, and that they frequently are neither of a carcinomatous nor a scirrhous nature; that by far the greater part commence by a very simple change, and that if attacked at a reasonable period after this commencement, they very rarely resist such therapeutic measures as were ployed to combat them at the Hospital of La Pitie. Within the last few years, a great number of females have been discharged cured from the hospital, without having undergone any operation, and since that time the afflux of patients with uterine disease has increased to a great extent. Nevertheless the number of operations has diminished, and I now perform amputation of the neck of the uterus once or twice a year, whereas formerly the same operation was performed fifteen or sixteen times during an equal period. Notwithstanding this great success, disease of the uterus sometimes resists all treatment, and removal of the neck becomes necessary as a last resource. Every surgeon knows that many women, yielding to a false shame, refuse to submit to an examination, and permit the affection to go on until an operation is indispensable; and I am compelled to observe, that although the new ideas on diseases of the uterus have already been laid before the public, yet that the old ideas are so rooted, that many physicians remain too frequently tranquil spectators of the progress made by the disease.

2. *Indications and Contra-indications for the Operation.*

We shall first explain the cases in which amputation of the neck of the uterus ought to be performed.

1st. When the cancerous state is well-marked, and too deeply seated to permit our trying cauterization.

2nd. When the disease does not extend beyond the superior part of the insertion of the vagina into the uterus.

3rd. Even when the existence of carcinoma may not be well established, I think we ought to operate, if the general health daily declines, and if the patient be not relieved by other therapeutic means employed, or if they do not prevent the disease from progressing in such a manner as would soon destroy all hope of a radical cure.

Is it not well known that certain non-carcinomatous ulcers of the lower extremity, by their very injurious influence on the economy, require the sacrifice of the limb; and why should not simple ulcers of the uterus exercise a similar influence on the constitution of the female? The uterus has a more powerful sympathetic influence on many other viscera. Experience proves that simple non-carcinomatous ulcerations of the uterus may become mortal, if not removed by the knife.

4th. We are generally recommended not to operate whenever there exists any engorgement of the body of the uterus. I think this opinion too exclusive, and would oppose it for the following reasons. Accoucheurs have proved that, in cases of extra-uterine pregnancy, the womb ordinarily presents double the ordinary volume. I have seen the same result from long-continued inflammation of the parts in the pelvis. Whenever the neck of the uterus becomes sufficiently diseased to render an operation indispensable, the body of the organ becomes more voluminous. I have been convinced of this by the autopsy of some females who died shortly after the operation. I have thus been convinced, that when, in the cases of which I speak, the uterus does not exceed double its ordinary volume, we have merely a simple hypertrophy, which need not embarrass the practitioner. This principle has been frequently sanctioned by experiment at the Hospital of La Pitie.

5th. Is the uterus more enlarged than we have just mentioned? We think that if the disease is accompanied by little pain, the existence of cancer is not certain, and that removal of the neck of the uterus should be attempted, because the woman, according to the view of physicians, is destined to a certain death. I have seen it succeed. The increased volume of the organ was produced by the simple white engorgement, which disappeared after the operation; but before we have recourse to this means, it is right to endeavour to restore the body of the uterus as nearly as possible to its normal state, and this we have often succeeded in doing.

6th. A question of high importance ought to fix the attention of the surgeon in a special manner. Cancer of the uterus is an affection which produces engorgement of the neighbouring organs less frequently than any other. Boyle long ago pointed out a circumstance which I have verified, and which supports the present idea, viz., that diseases of the uterus are less seldom carcinomatous than has been generally imagined; and, by the above reason, we are also enabled to explain why, in removing the neck of the uterus, and a certain quantity of healthy tissue surrounding it, we much more frequently obtain success than when cancer is removed in other situations.

7th. Is it necessary here to call to mind that I have proved, both by pathological anatomy and therapeutic reasons, that a

tumour supposed to be carcinomatous in its whole extent, was often merely so in its centre only. Hence the possibility of reducing it two-thirds before operating. Experience sanctions these facts.

8th. Is the operation admissible after the volume of the ovaries has become doubled? M. Larrey has shown us a patient whom he cured under these circumstances, and my practice has furnished a similar case.

9th. Surgeons in general reject the operation for cancer, when the diseased part is surrounded by engorged lymphatic ganglia which they cannot remove. The observations of Desault and Soemmering are in opposition to this precept; although when those great men wrote, the therapeutic ideas at present applied to indurations were not known. In the healing art, as in many others, we are apt to fall into exaggeration when we do not know how to distinguish cases. Whenever these lymphatic ganglia are few, of recent growth, little voluminous, and not adherent, we partake the opinion of Desault; and during the period that the Faculty of Medicine charged me with the duty of external clinique at the hospital of the school, I showed the Academy a patient in whom I had obtained the most fortunate results, by following the rules which I lay down.

10. In all cases where a viscus is affected, we are directed by surgeons not to operate until the organ has been restored to its normal state; but diseases of the uterus exercise a special influence on the chest, and the patients are frequently tormented by violent palpitations of the heart. Is the organ in this case altered in tissue, or is the affection merely nervous? The diagnosis appears to me very difficult, particularly in many cases; four practitioners most exercised in the use of the stethoscope, remained in doubt on a case of this nature at the Hôpital St. Come, where I had requested their assistance. The woman was operated on, cured, and has since lain in without accident. The extent of the present discourse does not permit me to treat the question of the hereditary nature of cancer and the cancerous diathesis. It now remains to describe the operation of removal of the neck of the uterus, but it will be well to show first how operative medicine is based on surgical anatomy.

THE SURGICAL ANATOMY.

It was once thought that the vagina was inserted into the neck of the uterus by a linear expansion. From this false idea of the anatomy of the part, it resulted that the surgeon, while operating, was always in dread of wounding the peritoneum; that the whole of the disease was in many cases not removed; or, finally, that the incisions were made too close to the affected portion, giving rise to so many examples of unsuccess, that amputation of the neck of the uterus was almost abandoned. I have shown that the upper portion of the vagina covers the neck of the uterus very extensively. In front there exists ten lines between the peritoneum and lower part of the neck; behind, the distance is at least ten lines. I have examined a vast number of bodies on this point, and never found those dimensions reduced when the neck remained in a normal state. It is unnecessary to call to mind that they are less in old women, in whom the uterus is atrophied. I do not speak of varieties in the length of the neck; these have been perfectly well pointed out by accoucheurs, but a fact of great importance has escaped the notice of physiologists, and it is by a knowledge of this fact that we explain the ready manner in which the uterus is brought down to the external orifice without producing any accident. Let a speculum be introduced into the vagina, and the superior orifice of the instrument placed round the neck of the uterus; desire the woman to make strong efforts as if she were going to stool, and the speculum will be pushed down by the uterus, which ordinarily descends within an inch or an inch and a half of the inferior opening of the vagina.

4. *Amputation of the Neck of the Uterus.*

Lauvariol, it is generally supposed, advised this operation in 1780; Osiander performed it with success in 1801, in the following manner:—Having placed the patient in a proper position, he traversed the neck of the uterus in two opposite points of its circumference, with curved needles armed with double threads; he then drew the uterus to the vulva by gradual traction, and removed the disease with the knife. M. Dupuytren has advantageously substituted the forceps of Museaux for the thread and needle employed by Osiander. I shall now explain the process to which he gives the preference, it is the only one attributed to him in the Treatise on Operative Medicine, published under his inspection in the year 1832. "The surgeon introduces a speculum into the vagina, and confides it to an assistant. This being done, he seizes and gently draws towards him, with a forceps of Museaux held in the left hand, all that portion of the neck of the uterus affected with carcinomatous degeneration, and which he removes with a double-bladed knife, or with a curved flat scissars,

very long, strong, and sharp. These are to be held in the right hand, and alternately carried upward, on the sides, and downward, turning their concavity inward, and making them act as much as possible on the sound parts, beyond the limits of the disease. The inconveniences attached to this process are the following;—In seizing the carcinomatous tumour, which is often very soft, it gives way, and the uterus cannot be brought down. Besides, M. Dupuytren says that he cut, with several instruments, *as much as possible* beyond the limit of the disease. These expressions indicate that the totality is not always removed. The instruments which he uses, acting in a narrow space, rather by pressure than cutting, give rise to most violent pain. The cancer can only be destroyed piece by piece; hence the diseased tissues are lacerated at the bottom of a narrow speculum, where they are masked by the blood, which prevents us from distinguishing them from the healthy tissues. This process, then, is only applicable to cases where we are prevented from sufficiently depressing the neck of the uterus, by adherences which may be formed. I have met with only one case of this kind.

M. Mayor, of Lausanne, has proposed to practise ligature of the neck with a porte-nœud and tourniquet invented by him. I merely quote this as an historic fact, for among the grave and numerous inconveniences attached to it, we have only to call to mind the accidents which arise when the ligature unfortunately embraces a portion of the uterus in tying a polypus. Messrs. Hatin and Colombat have invented mechanical instruments by which they remove the neck of the uterus. To reject these instruments, however ingeniously contrived, we have but to remember that their employment requires a painful dilatation of the vagina, that the cancer is often very voluminous, does not ascend always at equal points round the neck, and sometimes throws deep roots into the healthy tissues, &c.

5. *Method of M. Lisfranc.*

The patient is placed in the same position as for the lateral operation of the stone; a bivalve speculum is then introduced; this has the advantage of embracing the tumour better, and of putting the upper part of the vagina perfectly on the stretch, in which case the circular fold, which might mask the neck of the uterus, cannot be formed. The operator now cleans the os tincæ, in order to assure himself better of the disposition of the parts, if it be necessary. The forceps of Museaux, which is longer, more strong, and less curved, than those generally employed, is carried shut immediately below the organ. As soon as the branches are sufficiently opened to seize, if possible, two points of the neck diametrically opposite, the operator pushes gently on them, at the same time that he fixes them in the tissue of the uterus. This manœuvre is necessary, to follow the ascension of the uterus, which would otherwise cause us to seize it too low down, or perhaps miss it altogether. The speculum is easily withdrawn, because the forceps pass into the separation between its two valves. We are now to exercise slow and gradual traction on the uterus, in order to bring its neck below the inferior orifice of the vagina. It is unnecessary to mention that the traction should be made at first in the direction of the upper axis, and then in that of the outlet of the pelvis; but in order that the uterus should be more perfectly seized and brought down, and that all the points of the lower border of the neck should equally project below, the surgeon must apply a second forceps in a direction opposed to that of the first one.

This manœuvre has another advantage. Whatever may be the tendency of the uterus to ascend into the abdominal cavity during the resection, the tissues maintained in situ, may be divided either at the same, or at different heights, according to the circumstances requiring it. It is only by continuing traction for five minutes, or in difficult cases, for a quarter of an hour, if necessary, that we can obtain a sufficient degree of descent. The surgeon now carries his finger round the uterine insertion of the vagina, which is easily recognised by the presence of a kind of ring, above which he feels, on pressure, an empty space. Having cleaned the tissues, he confides the forceps to an assistant, placed in front of the pelvis. The operator himself is to the left of the patient. Having furnished himself with a curved bistoury cutting on its concave edge, and clothed with lint up to an inch of its blunt point, he directs the assistant to elevate the forceps, in order to give the uterus a kind of rotation, and expose better its posterior surface; by this means the limits of the disease are well seen, and the resection may be carried higher up.

The surgeon now glides the left index-finger behind the os tincæ, keeping the palmar surface towards the patient, and measures by it the height at which his incision is to be made. The bistoury is placed below this finger, which directs it, and serves as a point d'appui, while the assistant gradually depresses the forceps, and brings down in turn the other points

of the uterine neck. As the disease may extend itself higher on one side than the other, the assistant's duty is to manage the forceps in such a way as to incline the lower extremity of the uterus in a proper manner, that the whole of the diseased parts may be removed; he must also be careful to diminish the force of his traction as the incisions advance, or he may lacerate the tissues. The bistoury should be managed with a sawing motion to avoid injuring the great labia, or any dangerous deviation of direction, and to make the edges of the wound equal. The division is sufficiently difficult, from the great resistance of the tissue of the uterus.

But in some cases the volume of the parts does not permit them to be embraced by the speculum, and the surgeon cannot employ that instrument; here he must introduce simple hooks on the index-finger, and fix them either on the neck of the uterus, or on the most resisting points of the tumour. It has been proposed to introduce certain instruments into the uterus, which, by a mechanical contrivance, expand within the organ, and thus tend to depress it. I have never tried them; they produce a contused wound, which is not removed with the diseased parts, and which must expose the patient to the well-known dangers of traumatic inflammation. If a soft fungus existed, though of small size, the introduction of the speculum might give rise to a hemorrhage, and thus render it impossible to see the neck of the uterus, in spite of injections, cleaning the surface, &c. In this case it will be better to renounce the use of the speculum, and be guided by the principles already laid down. However, as the double-branched speculum has entered the vagina, its two leaves might be widely separated, so as to allow the fungus to become engaged in the upper part of the instrument without being rubbed. Dr. AVENEL reports a case of this kind, which I showed at the clinique of LA PITIE.

A large tumour, ascending high up on the neck of the uterus, is not easily brought down through the vagina, and does not permit us readily to see its limits, or attack it with certainty. In a case of this kind, which is very grave, and which I have often met, I divide the fold of skin and mucous membrane, which, as I have lately demonstrated, gives the great extent to the perineum in its antero-posterior diameter. When cancer of the neck of the uterus had extended deeply into the body of the organ, surgeons, after having removed the superficial portion of the diseased parts, were in the habit of employing cauterization, a means very dangerous, and very rarely indeed attended with success. In such cases I thought of practising two semilunar incisions, which are united at the extremity, and of which the longer diameter is antero-posterior; by this means I dissect away all the diseased parts from the depth of the organ, and have obtained very fortunate results. In fact, pathological anatomy had convinced me, contrary to the general opinion, that even in cases where the extent of the disease did not permit me to remove the neck of the uterus, the body of the organ was almost always healthy, if we except some slight tumefaction due to a simple hypertrophy; I have shown preparations proving this, frequently, at the hospital. One shudders to think of the violent pain which this operation must produce; but here is a vulgar error which it is important to remove. If the uterus be very sensible, even to moderate pressure, nature has fortunately rendered it insensible to the blade of a cutting instrument; this is so true, that many women, operated on at LA PITIE, thought the operation was not commenced, even when the whole neck had been removed. Besides, we know that patients are not conscious of any pain produced by leeches applied to the lower part of the uterus.

6. Accidents which follow the Operation, and Means proper to heal the Wound.

In looking over what has been written, it is easy to see that with the exception of plugging and metro-peritonitis, this point of pathology and therapeutics has not been touched upon at all. Surgeons were too much impressed with a fear of hemorrhage; they had at once recourse to plugging; thus the uterus was unable to get rid of the blood brought to it by the irritation of the operation; and this powerful cause of inflammation, joined to the pain occasioned by the presence of the plug, often gave rise to a metro-peritonitis, which was generally fatal. When the woman has not been reduced by previous hemorrhage, I let the blood flow away if it does not come too fast, and do not usually arrest it before she has lost twelve or fourteen ounces. In following this rule, I have not been obliged to plug more than six times in ninety-nine operations, and the plug moreover was generally applied to the lower part of the vagina only, and was withdrawn in one or two hours. I have never lost a patient from hemorrhage; three only have died in consequence of metro-peritonitis.

The extent to which the present observations have run does not permit me to describe the nervous accidents; it is sufficient to say that they appear frightful to a

surgeon who sees them for the first time, but they are not dangerous; after a few hours they become calm, and are removed by an antispasmodic julep. The absence of sudden suppression of the bleeding often determines pain in the pelvis; in this case I remove the clots contained in the vagina, and throw up warm emollient injections. If the blood begins again to flow, the pains usually disappear; if not, I cover the lower part of the abdomen with a poultice sprinkled with laudanum, and practise a revulsive bleeding of the arm.

Metro-peritonitis when well-marked is to be treated according to the established rules.

A small revulsive bleeding from the arm is also the best means of combating the consecutive discharges of blood which may exist either with or without uterine pain.

The rules of diet are too simple to deserve any particular notice.

As soon as all danger of hemorrhage is removed, it will be advantageous to inject into the vagina a small quantity of mallow-water nearly cold, in order to remove any sanguineous clots which may remain, and which putrify with great rapidity.

The wound, however, made in the uterus, is not favourably circumstanced for a quick cicatrization, for,

1st. It is constantly bathed by liquids more or less irritating.

2nd. The depression of the uterus exposes it to be rubbed against and injured.

3rd. The organization of the uterus does not permit its edges to come in close contact; hence arises a pellicle of cicatrization which covers a large surface, and requires a long time to be perfectly formed.

The injection of emollient and then of stimulant fluids, absolute rest, and the cauterization of the wound with the liquid proto-nitrate acid of mercury, should be employed according to the indications. Six or eight weeks are necessary to obtain the complete cicatrization of the wound. I cannot enter into the important question of the relapse of this disease, on which modern surgery, taking medicine as its basis, has thrown great light; and I must also pass over certain inconveniences which many women experience after their cure, but which are dissipated soon by a very simple treatment. The following, however, is an extremely important point of physiology. Amputation of the neck of the uterus proves that this part of the organ is not indispensable to pregnancy, which may take place and terminate fortunately without it. In general, labour is more easy. In ten patients who had become pregnant after the operation, one only was delivered at four months; but she had been guilty of imprudences capable of producing abortion under any circumstances. I may add that Madame Carpentier, who permits me to name her, has carried two children to the full period. Amongst the great number of women operated upon, I know only one in whom the cicatrix has completely obliterated the orifice of the uterus. She menstruated regularly, and in the ordinary quantity; but each period was preceded and accompanied by severe nervous accidents, and some symptoms of metritis, which were soon dissipated. Besides, she never exhibited any signs of sanguineous accumulation in the uterus; the menstrual discharge came from the surface of the vagina. This woman died. On examining her, we found some lymphatic ganglia engorged in the pelvis; a chronic abscess under one of the psoas muscles; no metritis or peritonitis; but it was evident that the lower orifice of the uterus was obliterated. The introduction of a probe from time to time would prevent this accident. In cases where it did take place, could we remedy it by the trocar or bistoury? Experience must decide that question.

In drawing this lecture to a close, I have merely to remark, that of ninety-nine females in whom I have removed the neck of the uterus, fifteen are dead, and eighty-four are cured, and in the former the disease was very considerably advanced. We therefore may hope, that as the new ideas on uterine disease become more extended, surgeons will be induced to operate sooner, and thus the chances of failure will be considerably diminished.

Since this lecture was written, I have performed the amputation of the neck of the uterus three times. One patient is dead, the two others are under treatment. —*French Gazette*, published June 20, 1834.

AN ESSAY ON

THE ATOMIC THEORY.

By JOHN S. HILEY, *Esq. Surgeon, Elland.*

PART II.

IT was in 1808, an era the most eventful in the annals of chemistry, that this great man first published to the world his "New System of Chemical Philosophy," in which was contained a brief view of his notions respecting atoms and definite proportions. It appears, however, from some observations made by Dr. Thomson in the historical introduction to his "First Principles of Chemistry," that he entertained these ideas much earlier, for in that work

the Glaswegian Professor observes, "I first visited him (Mr. Dalton) at Manchester, on the 26th of August, 1804, and on that day he explained to me his notions respecting the composition of bodies." Thus, this was four years earlier than the period when he made his invention generally known, and, consequently, from 1804 the chemists of a future season will probably date its origin. In the last chapter of the first volume of the work alluded to, he has given us a brief account of his theory, and recorded its tenets in such simple colours, as to have greatly surprised philosophers that the idea was not hit upon earlier. Previous to this period, however, Dr. Thomson had published in the third edition of his "System of Chemistry" a sketch of Mr. Dalton's theory; and in the same year, 1807, Dr. Wollaston showed that the proportions of the acids in the three oxalates of potash, viz., the oxalate,—the binoxalate,—and the quadroxalate, were as the numbers 1,—2,—4. These with a variety of other facts drew the attention of men of genius to the subject, and amongst the rest, the deservingly celebrated Sir Humphrey Davy, and to crown all, in the succeeding year, the opinions and views which were held by the discoverer himself, were received from his own pen.

From the simplicity of the principles on which the corpuscular theory is founded, it has always been a matter of infinite surprise to me that it was not invented earlier. And this surprise has been much heightened by the fact, that ever since I knew how to think, indeed long before I could possibly have any idea of the system in question, I imagined that all matter must be united in definite proportions. This idea was confirmed by a knowledge that the natural productions of the present day are exactly similar in their appearance and properties to the products of former ages; that the trees of the forest have every similarity in their trunks, branches, leaves, flowers, and seeds, to those which existed amongst the earliest of our ancestors; and that the seeds of the various herbage never produce any other vegetable than a specimen of that particular family to which they belong. I examined the daisy of one season, and found subsequently that it did not vary a shade from that of the next; and every succeeding year confirmed the truth of the observation. I found that all chemical salts, whether formed by nature or by art, in the sea, in the bowels of the earth, or in the laboratory of the chemist, were exactly similar in their appearance and properties. I found that they were made up of precisely the same portion of materials, and that in every place they assumed the same outward character. The atmospheric air which occupies the regions over the deserts of Africa, or blows with such a freshness on the Andes, does not vary more in its composition from that which we breathe in the sunny climes of England, than does the water of one well differ from that of another. The law of similarity of composition seems to be the same everywhere, and the unchangeableness preserved by elementary substances is as apparent in this age as with the inhabitants of the antediluvian world. However I might try to alter the nature of matter, my attempts were as eternally thwarted as those of the alchemists of old, for, I observed that it was easier to turn the sun from his course than to change the nature of the minutest particle.

Hence I was led to conclude, that if nature preserved so strict a resemblance in the aggregate of her productions, the like law must extend to all her ultimate molecules. I naturally imagined that if this were not the case, the properties and the appearance of her various products must be continually changing; that if the atoms of which they were composed were not indivisible and immutable, but admitted of infinite division, the surfaces of those atoms, if such they might be termed, would become abraded, disfigured, and worn by use, so that instead of the aggregates of them maintaining their usual similarity of form, they would after a season participate in the common injury.

Nothing will better illustrate this fact than the following passage: A number of atoms in the shape of minute cubes to make a larger cube, if abraded and worn by use as they naturally would be, providing we admit the infinite divisibility of matter, would not then form an exact cube, every trial would find it more and more out of square; it would become deficient first in one part, and then in another, till at last it no more resembled a perfect cube than a circle resembles a square. Would not this be the case with natural productions? Would not the particles entering into their composition be constantly suffering abrasion? so that scarcely two things would be formed alike. Where would be the harmony that now exists? The nodding oak, whose symmetry of form has attracted the attention of men in all ages, would no more resemble itself, than the gnat resembles the elephant. Abrasion of its particles might cause it to become stunted, shrivelled, and puny, so that instead of our woodlands being adorned with specimens of vegetable growth, such as existed when

the particles which formed them came from the hands of their Maker, the inhabitants of modern times would behold a race of disfigured, worthless, puny shrubs. Neither would this change be confined to the vegetable world. Both the animal and mineral kingdoms would suffer as well, and what was fine and well-proportioned in this age might be shrivelled and deformed in the next. To admit the infinite divisibility of matter, we are also compelled to acknowledge the impossibility of all natural productions remaining the same in a succeeding as in a previous age, for consequent upon this law is the fact that matter infinitely divisible must constantly be enduring a greater or lesser change in itself.

Anyhow, supposing the atomic theory should be hereafter proved to be not only untenable but unphilosophical, we are no losers in supporting its tenets. For if, mathematically speaking, a particle possessed of surface cannot otherwise than admit of being divided into parts, might we not argue that an atom, supposed like any other body to be composed of particles, is rendered indivisible by the superior affinity exerted betwixt the particles of that atom? This would at once relieve us from the necessity of supposing, in opposition to all mathematical demonstration, an atom possessed of length, breadth, and thickness, incapable of being subdivided in case we had the means, or that the attraction present therein was dispelled. So satisfied am I of the truth and sublimity of this law, that I now consider it to rank in point of importance with the laws of specific gravity, and the attraction of gravitation.

It will now be necessary to state the nature of Dalton's views of the atomic theory, and likewise to notice the improvements and modulations which this system has undergone from the other chemists who assisted in promulgating and extending it.

This invention was first published to the world in 1808, in the first volume of Dalton's "New System of Chemical Philosophy." The year previous it had attracted considerable attention, in consequence of the papers of Wollaston and Thomson; but on the publication and explanation of its laws by the inventor himself, the eyes of almost every philosopher in Europe were drawn towards it. There was a general movement in the ranks of chemistry, and a much stronger sensation created amongst its followers than was the case when Sir H. Davy discovered the nature of chlorine, or determined the bases of the alkalies and earths. Dalton's suppositions, however, were not grounded so much on the truth of the old atomic hypothesis, as on the fact, that bodies united in definite proportions. He did not labour to support the important law that atoms were indivisible, further than was necessary for the true exhibition of his own theory, but, like many besides him, he was a firm believer in that law. He argued, that when combination takes place betwixt two bodies, the particles of these bodies unite in the proportion of 1 to 1, 1 to 2, 1 to 3, &c., and from this fact he thought that the relative weight of the atoms of each substance might be deduced. The Daltonian system has been called by different names, as the "doctrine of definite proportions," "chemical equivalents," "proportional numbers," &c.; but owing to the ingenious hypothesis which its discoverer introduced into his paper, on the possibility of bodies uniting in indivisible atoms or particles, this happy invention has been generally termed the "atomic theory."

But as this portion of the doctrine has not been so universally received, it would probably be advisable to style it the "law of definite proportions," as not only being more applicable to the strict truth, but better suiting the genius of every variety of readers. From this doctrine of definite proportions then, according to Dalton, spring three laws, the truths of which are not founded on any speculation, but established and determined from the results of a vast number of experiments, which it would be almost folly in any one to oppose.

The first of these propositions is, that whenever two bodies combine, the union is generally in fixed and invariable proportions. The second, that the proportion in which one substance unites with a second, is always the same in its union with a third. Thus 1 part of hydrogen by weight unites with 8 of oxygen and 6 of carbon; and 6 of carbon combines with 8 of oxygen, from which it will be learnt, that the numbers 1, 6, and 8, are the combining proportions of hydrogen, carbon, and oxygen. The third and last proposition is, that when combination takes place in more than one proportion, the quantity of one substance is generally double, triple, or some multiple of, its lowest combining number. Thus the equivalent number of sulphur being 16, and that of oxygen 8,

Sulphur	16,	Oxygen 8,	will form hyposulphurous acid.
	16,	16,	sulphurous acid.
	16,	24,	sulphuric acid.

Here the quantity of oxygen in the sulphurous and sulphuric acids is double and triple its proportion in the first and lowest combination. The combining number of sulphur admits of the same change; thus there are are two combinations of sulphur and hydrogen,

Hydrogen .... 1, Sulphur .... 16, form sulphuretted hydrogen,
1, 32, form bisulphuretted hydrogen.

In this, as in the former instance, the quantity of sulphur, in its second combination with hydrogen, is precisely double that of its proportion in the first.

These three propositions, however, deserve further illustration. The first, that whenever two bodies combine, the union is generally in fixed and invariable proportions, admits of some slight modifications, as has been noticed in a former part of this essay, viz., in the instances of the union of the stronger acids with water, water with alcohol, &c. Here the union is not definite, for a drop of water will mingle with a gallon of acid, or a gallon of acid, or any other intermediate quantity from less than a drop, with every proportion of water. The knowledge, too, of bodies uniting in more than one proportion, might be adduced in proof of the non-universality of this law; but then, when we consider that these quantities are almost invariably either double, triple, or some other multiple of the lowest equivalent number, and not an intermediate quantity, this objection immediately disappears. Instances of this nature have been already given, to which others might be added, thus:—

Hydrogen .. 1, Oxygen .. 8, form protoxide of hydrogen or water.
1, 16, form deutoxide of hydrogen.

Here the equivalent of oxygen in the deutoxide of hydrogen, instead of an intermediate number, is an exact multiple of that in the water. In the combinations of oxygen and carbon, the same law prevails; thus:—

Carbon .... 6, Oxygen ....8, form carbonic oxide.
6, 16, carbonic acid.

Nothing, in the whole range of chemistry, is perhaps more striking than the almost universal presence of this canon throughout all chemical compounds. It prevails so generally, that it would be preposterous and absurd in any one to attribute its existence to chance alone, and of such importance is its nature, that we may probably impute the constant harmony and similarity in all natural as well as artificial productions, purely to its fixed and unchangeable character. Indeed, nothing can be more necessary to the beauty and harmony of nature, for were it altered by a decree of Providence, have we not every reason to believe that the aspect and nature of the globe would be materially changed? And is it likely that this change could be for the better, for where so much beauty already prevails, no one, I presume, can imagine that it could be much improved. Were different proportions of the two gases which form our water to be substituted for those by which it is now produced, where or how might the raging thirsts of summer be quenched? In no other proportions will oxygen and hydrogen form this useful beverage, and in case of an alteration in its constituents, the consequences must be manifest to every one. The ground would be parched, vegetation would wither, every living thing would die, and our springs and rivers would be dried up. Again, if the constituents of atmospheric air were to be changed, what would be the results? Instant suffocation, or a short feverish existence. It was a change of the former kind which caused so destructive a mortality in the celebrated Black Hole at Calcutta, and innumerable instances are on record of certain death ensuing from the inspiration of oxygen in an undiluted state. The purity of atmospheric air is as necessary to vegetable, as to animal life, for where its decomposition has been affected or otherwise rendered impure, vegetation has invariably suffered. In a pure state it supports combustion, in an impure one it extinguishes flame. Indeed, so necessary is its purity to the support of the vegetable and animal economy, and combustion, that were it otherwise, respiration and combustion would cease. Inasmuch as the existence of definite proportions is requisite in these instances, so the experiments of unnumbered philosophers have demonstrated, that this law is equally indispensable to the existence of all natural and artificial productions, for were it changed, the properties and appearance of the variety of matter now in being must change also, and, in all likelihood, as Shakspeare sublimely observes,

> The cloud-capp'd towers, the gorgeous palaces,
> The solemn temples, the great globe itself,
> Yea, all that it inherits, would dissolve.

From substances uniting in definite proportions, we are enabled to form compound bodies synthetically, and, in case of analysis, can correct any errors arising from deficiency in manipulatory skill, which otherwise would have escaped our notice. Were we about to decompose a carbonate of lime, we know how much carbonic acid ought to be evolved; thus, if from 50 grs. of the carbonate of this earth, we did not obtain 22 grs. of carbonic acid, we might then conclude that either the experiment was faulty, or that our conclusions were erroneous. On the other hand, did we wish to form muriatic acid, our object would be to mix a volume of hydrogen equal 1, with a volume of chlorine equal 36. This is obvious, from the fact of 1 and 36 being the combining proportions of these gases necessary to the formation of muriatic acid. As in the instances before alluded to, an atom or volume of chlorine gas is 36 times heavier than an atom or volume of hydrogen. Under this head it will be necessary to give the definite or combining proportions of a few elementary substances, and to point out the manner in which they were first deduced. I have already alluded to the chemical equivalents of a few bodies, but it will be necessary again to recount them. At the same time, for the better explanation of the subject, such have been pitched upon as are most familiar to us.

Chemical Equivalents.

Hydrogen	1.
Carbon	6.
Oxygen	8.
Phosphorus	12.
Azote	14.
Sulphur	16.
Sodium	24.
Chlorine	36.
Potassium	40.

It may be noticed here, that any number may be taken as the standard of comparison. If hydrogen be fixed upon, the above will be the weights or definite proportions of the rest; whereas, if oxygen be chosen as unity, of course the relative value of the others will be diminished 8 times. Thus—

Hydrogen	0.125.
Carbon	0.75.
Oxygen	1.00.
Phosphorus	1.5.
Nitrogen	1.75.
Sulphur	2.00.

Different chemists have chosen different standards, and have likewise altered their value. Dalton, Henry, and Davy, pitched upon hydrogen, on account of its being the lightest of all known bodies, whilst Thomson, Wollaston, and Berzelius, selected oxygen, because of its almost universal presence in all important combinations. The opinions of these celebrated chemists disagree also in the relative value which each has attributed to his different standard. Thus, according to Thomson, the atomic weight of oxygen is 1,—according to Wollaston 10,—whilst Berzelius rates it as high as 100.

This difference has, at one time and another, created considerable confusion, though now, when the system is so well understood, it is of little or no importance, since in the one case, we have the advantage of whole numbers, whilst in the other we are merely compelled to have recourse to fractions. But, as fractional parts, however small, are always attended with difficulties from which integrals are exempt, the disadvantages attending Thomson and Wollaston's numbers will be immediately observed, whilst those of Berzelius, besides being exposed to this evil, are far too high for ordinary talents to comprehend, and what is worse, the integrals themselves being high, of course the fractions are complicated in a corresponding degree. Some chemists are disposed to think with Dr. Thomson that when the scale of numbers is low, the fractions will not be more difficult to retain than high integral parts. Thus they argue that it is easier to recollect 4.5, the weight of an atom of chlorine according to Thomson, than it is to remember the integral 36 of Dalton's scale.

Now I doubt this, for in my opinion it is easier to bear in mind any whole number, however high, than to think of the lowest fraction. Nor is there, to my thinking, a greater effort of the memory requisite to recollect 200 than 25, the former being the weight of an atom of gold, according to Dalton, and the latter the weight according to Thomson. Add to this, a worse evil attends the method of the Glaswegian professor. Where there are fractional parts joined to so many different quantities, a repetition of the same fraction could hardly be avoided, thus the number 5, 25, 75, occur repeatedly, and I imagine no one will be disposed to deny that when this is the case, the difficulty is by any means decreased.

Now Dalton's numbers are altogether free from these objections. He pitched upon hydrogen as unity, from its lightness, and from its combining in the smallest quantity, and, what is surprising, all the atomic weights of the other elements are multiples of it by a whole number. This is a great advantage, as we are enabled at once to dispense with all fractions, and all other substances being compared with a

body lighter than themselves, we have only an ascending, instead of a descending and an ascending scale. Add to this that the experiments of other chemists, and more particularly those of Dr. Prout, have rendered it probable that hydrogen is not only the lightest element known, but the lightest body in nature, therefore, if future researches should prove the truth of this, all chemists must, with Dalton, necessarily adopt it as the great standard of comparison, so that instead of variety of opinion prevailing on this subject, one grand and unalterable law will ultimately exist throughout every country where chemistry is cultivated.

Now it was noticed in the atomic theory that if a standard could be pitched upon, it would be no difficult matter to determine the chemical equivalents of all other elements. Accordingly Dalton fixed upon hydrogen, and called it unity, and compared the combiningw eights of all other substances with this gas. The weight of hydrogen was deduced from its union with oxygen to form water. It was found that 2.117 grains was the weight of 100 cubic inches of hydrogen, and that 33.888 grains was the weight of 100 cubic inches of oxygen. On passing the electric spark through a mixture of these gases in similar proportions, it was discovered that whilst the whole of the hydrogen was condensed, but 50 cubic inches, or 16.944 grains, of the oxygen disappeared. Comparing the volume of these gases we find, that, instead of oxygen being only eight times heavier than hydrogen, it is sixteen times the weight. But the atomic weight of oxygen, instead of being 16 is only 8. The reason is this. I have shown that 100 cubic inches of hydrogen weighing 2.117 grains, can condense no more than 50 cubic inches of oxygen weighing 16.944 grains, and as the atomic weights of all elements are determined from the proportions in which they enter into combination with other bodies, the same law must necessarily extend to oxygen. Hence if 2.117 grains of hydrogen can combine with no more than 16.944 grains of oxygen; or 4.234 grains of hydrogen unite with 33.888 grains of oxygen, then the relation in which the latter stands to the former will be as 8 to 1; for as 33.888 : 4.234 :: 8 : 1. From this we must conclude, that if the oxygen and hydrogen in water unite atom to atom, the atom of oxygen must be 8 times heavier than the atom of hydrogen, since 33.888 and 4.234, when reduced to their lowest terms, are as 8 and 1; and the former is the weight of 100 cubic inches of oxygen, and the latter the weight of 200 cubic inches of hydrogen, these being the proportions in which they combine to form water.

There are other rules for deducing the equivalent numbers of the simple elements, besides the one just recounted. Thus the atomic weights of oxygen and hydrogen may be derived from the specific gravities of those gases. Now the specific gravity of hydrogen being 0.0694, and that of oxygen being 1.111, by dividing the latter sum by the former, we obtain the atomic weight of oxygen. This law, however, may be better demonstrated by the following process:—Let us take x to represent the weight of the atoms in a measure of hydrogen, then as $0.0694 : 1.111 :: 2x : 16x$. By reversing this process we have the atomic weight of hydrogen. Thus, as $1.111 : 0.0694 :: 16x : 2x$. Now $16x$ is the relative weight of the atoms in a volume of oxygen, and $2x$ represent the weight of an equal number of the atoms of hydrogen. When, however $16x$ and $2x$ are reduced to their lowest terms, they are as 1 to 8, for as $2x : 16x :: 1 : 8$. Hence we conclude, that an atom of oxygen is 8 times heavier than an atom of hydrogen.

There is another method of determining the relative weights of the atoms from the specific gravities of their gases—viz. by comparing the sp. gr. of each gas, with the weight and sp. gr. of atmospheric air. Atmospheric air is considered unity, thus,

1.000 : 1.111 :: 30.5 : 33.8.88 ..... Weight of oxygen.
1.000 : 0.0694 :: 30.5 : 2.117 ..... Weight of hydrogen.
1.000 : 0.9722 :: 30.5 : 29.652 ..... Weight of nitrogen.
1.000 : 0.4166 :: 30.5 : 12.709 ..... Weight of carbon.

Now 1.000 represents the sp. gr. of air, —1.111 that of oxygen,—30.5 the weight of 100 cubic inches of air,—and 33.8.88 the weight of 100 cubic inches of oxygen. The exact weight of 100 cubic inches of the gases being made out by this rule, we have only to compare the products with the weight of the same measure of hydrogen, which, on account of its lightness, was assumed by Dalton as the standard thus, as

2.117 : 33.888 :: 1 : 16.
2.117 : 29.652 :: 1 : 14.
2.117 : 12.709 :: 1 : 6.

This important law, besides extending to the two last elements, is observed to be present in other principles when in a gaseous state. It will, however, be seen that oxygen, strictly speaking, forms an exception, being 16 in place of 8. This discrepancy, as has been before alluded

to, arises from oxygen combining with hydrogen in the proportion of 50 cubic inches of the former to 100 of the latter, instead of equal measures of each element. Hence, in this instance, the law must undergo some slight alteration. This may be done by doubling the volume of hydrogen, or by halving that of oxygen. Thus we may either take the weight of 200 cubic inches of hydrogen—that is, 4. 234, or the weight of 50 cubic inches of oxygen—viz. 16. 944. If the former, it will be as follows:—

4. 234 : 33. 888 :: 1 : 8.

If the latter, it will be thus, as

2. 117 : 16. 944 :: 1 : 8

In either case the chemical equivalent of oxygen is 8. The latter method is perhaps preferable, since, instead of two measures of hydrogen, we have only one.

The atomic weights of the two gases, hydrogen and oxygen, being determined by one or other of the three preceding laws, chemists have it now in their power to discover the definite proportions of other elements. Thus the equivalent number in which sulphur united with oxygen, being twice as heavy as the latter element, its atomic weight was fixed at 16, in reference to hydrogen; for oxygen being 8 times heavier than hydrogen, sulphur, if as heavy again as oxygen, must of necessity be 16 times heavier than hydrogen. Hydrogen is the standard of comparison for determining the proportionals of all simple substances. In this way we have the relative weights of the atoms of phosphorus, azote, iron, potassium; and the other elements have been ascertained. Having arrived at this point, it was not difficult to determine the definite proportions of the compound bodies. On the discovery of a new substance, the experimentalist had only to find out in what quantity it united with another, the equivalent of which had been previously ascertained, and then it was easy enough to calculate in what proportion it would combine with all the rest. The Glaswegian professor, Dr. Thomson, was much indebted to the immutability of this law, when he engaged himself so assiduously in improving and extending the atomic theory. His statements are supposed to be very accurate, and it may not be superfluous to remind the student, that the professor determined the weights of an atom of the several elementary substances from their union with oxygen, or with hydrogen, or with both.

Till very lately, sulphur was considered by all chemists as a simple element, and, as such, its equivalent number, deduced from its union with oxygen, was placed at 16; but the experiments of Mr. J. M. Corbett, inserted in the 515th number of the *Mechanic's Magazine*, seem to prove that it ought not to rank amongst elementary substances any longer, for, according to the statements of that gentleman, he has effected its decomposition by means of lightning. I will transcribe the passage: "I (Mr. Corbett) enclosed some sulphur in a glass tube of two feet long by one inch in diameter. I passed a very fine spiral wire through the sulphur, and then fixed the whole in a metallic lightning conductor, which was insulated above the sulphur apparatus. The glass was so contrived, that any air coming from it would pass into a receiver placed for its reception. I now waited for the lightning to pass down the rod, and on visiting the spot, I found the spiral wire fused, and the lower part of the sulphur changed into a powder as white as snow, and my receiver full of hydrogen. I have named this new substance thiogen;* its sp. gr. is 1. 707. It has a great affinity for hydrogen, and converts muriatic acid into chlorine. It converts oil and fat into carbon in quite a new state, the carbon being white, soft, and nearly transparent, after having lost its hydrogen. Thiogen decomposes phosphorus by depriving it of hydrogen; the remaining part is a new and very inflammable gas, of the colour of chlorine."

In all likelihood this decomposition of sulphur will affect chemistry considerably. The atomic weight, the theories, and the nomenclature, will severally require correcting, and probably the discovery itself will lead ultimately to the decomposition of other bodies hitherto considered simple. By means of thiogen the metals may possibly be decomposed, more especially arsenic, since the latter, besides being isomorphous with phosphorus, resembles it very closely in its other properties.

Nothing was more gratifying to me than the disclosure of this important fact, for it has often occurred to my mind, that could we anticipate the discoveries and researches of future chemists, we might behold the 55 substances, at this period considered elementary, reduced perhaps to fewer than half a dozen. I have imagined, from the almost invariable diffusion of the three elements, hydrogen, oxygen, and carbon, in all species of matter, that we must view in them the three great agents by which the countless varieties of mineral, vegetable, and animal substances, are formed. Sir Humphrey Davy, if he did not extend his views so far, certainly thought that considerable changes would

* From θειον, sulphur, and γενναω, I generate.

be effected by future chemists, and that the number of now-supposed simple bodies would be greatly diminished. Who would be surprised if nitrogen should be decomposed, or chlorine, iodine, and bromine, found to be nothing more than peculiar modifications of the two great elements, hydrogen and oxygen?

The like fate probably awaits several of the metals. Every year will supply us with more powerful agents, and more perfect instruments of research, through whose assistance many facts may be brought to light "which are not dreamt of in our philosophy." At one period fire was considered the most powerful agent which chemists were known to possess, and until electricity came to be understood, it maintained its rank as such in the opinions of the scientific world. Electricity then arrived at perfection, but the discovery of Galvani predicted its early decline. At length the improvements which galvanism underwent under Davy, the various purposes to which it was applied, and the power it was discovered to possess, claimed for it, in its turn, a pre-eminent place in the investigations of men of science. Hitherto it has been looked upon as the most extraordinary power in existence, and of course, like fire and electricity, will always be used by the experimentalist, but in all likelihood the researches of Mr. Corbett have now directed the attention of chemists into a new and more wonderful channel, where they will find a stronger and more useful agent even than galvanism. In this way we are gradually advancing towards perfection, and as such there is no telling what revolutions in all chemical theories a very few years may produce. Probably, could the sages of ancient Greece look down upon the present improved state of natural and experimental philosophy, when compared with its progress in their own days, they would not be more surprised than would the shades of Lavoisier, Priestley, Berthollet, and Davy, might they be permitted to visit the laboratories of chemists a century hence.

I am now deviating. After Dalton, the most deserving labourers in the atomic philosophy are, Thomson, Wollaston, Prout, Davy, Berzelius, and Gay Lussac. To the first our gratitude can never be sufficiently expressed, as, through his exertions in the "Annals of Philosophy," &c., the scientific world was made acquainted with many interesting facts relating to the newly invented system. He appears to have been a most laborious workman, and the patience, talent, and tact displayed in the course of his chemical investigations, may be truly held up as a pattern to all future experimentalists.

Nor must the name of Wollaston be passed over without a eulogium. In his synoptic scale of chemical equivalents, he has presented chemists with a most valuable instrument. Whilst it is a kind of guide to the young experimentalist, it serves to correct and obviate the errors of the manipulations of the philosopher. It presents to the eye a mass of information of which those can form no conception who have not had an opportunity of examining it. It points out the element which will decompose any other compound body, the proportion requisite to effect the decomposition, and the quantity of salt &c. resulting therefrom. The principles on which Wollaston's scale was invented, are the following. It was noticed long ago, that when two neutral salts decompose each other, the products themselves are neutral. This is invariably the case, for under all circumstances and situations 91 parts of carbonate of soda require for their decomposition 94 parts of muriate of lime, after which the resulting salts are neutral." The cause of this important phenomenon may be traced partly to the same compound body always being of a similar composition, and to the fact that the carbonic acid possesses exactly the same power of combination and saturation as the muriatic acid.

This is rendered more evident by the circumstance that the 48 parts of lime contained in the neutral muriate of lime, equally neutralize the 38 parts of carbonic acid contained in the carbonate of soda, and that the 53 parts of soda present in the latter salt, exactly saturate the 46 parts of muriatic acid present in the former. Similar laws extend to the acids and bases of all other neutral compounds. Thus 40 parts of sulphuric and 54 of nitric acid, will both neutralize 32 parts of soda and 78 parts of baryta; consequently, if 72 grains of sulphate of soda in solution be mixed with a solution containing 132 grains of nitrate of baryta; two neutral salts are the result, the one soluble and the other insoluble. The soluble one, nitrate of soda, is equivalent to 86 grains, and the insoluble one, sulphate of baryta, is equivalent to 118 grains. Now these numbers 40, — 54, — 32, — 78, &c., are termed equivalents, and being always in the same proportions to form or to effect decompositions of the neutral salts, it is easy to see how they might be arranged on the logometric scale of Gunter. In explanation of this scale it will be necessary to remark, that on examining it, and pitching upon a certain number, we shall find that there is precisely the same

distance from half that number that there is between it and double its amount. Thus on measuring with a pair of compasses we perceive that the distance from 10 to 20 is equal to that from 20 to 40, and that the space from 40 to 80 is the same as from 80 to 160. This is the case with all the other numbers marked on the scale, for on whichsoever we may fix it will be seen, that that number has the same relation to another at any distance above it, that the number at the same distance below has to the first. For instance, let us fix upon the number 24, and afterwards take a second number above it, as 16; it will then appear that a third number at the same distance below 24 will have the same relation to 24 that 24 has to 16. This will be 36, for as

$$36 : 24 :: 24 : 16.$$

It is not difficult to see how a scale on this principle might be applied to the chemical equivalents of both elementary and compound bodies, and Wollaston accordingly adapted it to this purpose. A slider, with numbers upon it, from 10 to 320, distanced in the manner above explained, was first formed, and on each side of them were placed the names of substances, both simple and compound, in juxtaposition with the numbers representing their equivalents. Thus, when the slider is in its proper position, 10 (which Dr. Wollaston fixed upon as the atomic weight of oxygen) will be opposite that element,—20 opposite sulphur,—and 40 opposite copper. Now sliding the scale to any other part, the sulphur and copper will stand opposite a number bearing the same relation to oxygen in point of quantity, as is the case in the above. Thus by bringing oxygen opposite 20 on the scale, or any other number, 40 will be opposite sulphur, and 80 opposite oxygen, for as

$$10 : 20 :: 20 : 40,$$—or as $10 : 40 :: 20 : 80.$

Another great use of this scale is, that if we wish to ascertain how much of a neutral salt will decompose another, we have only to adjust the scale accordingly. Thus, if we had 90 grains of sulphate of soda, and wished to know what weight of nitrate of baryta would exactly decompose it, we have only to bring the number 90 opposite sulphate of soda, when on looking for the nitrate of baryta we shall find the quantity requisite, viz. 160 grains. Would we know how much baryta is contained in 100lb. of the sulphate of that substance, our object would be to bring the number 100 opposite the sulphate, after which, on looking for barytes, the information required is obtained.

The power of such a scale is further illustrated in a passage from Dr. Wollaston's own writings. "If, for instance, the salt under examination be the common blue vitriol, or crystallized sulphate of copper, the first obvious questions are,—1. How much sulphuric acid does it contain? 2. How much oxide of copper? 3. How much water? He (the analytic chemist) may not be satisfied with these first steps in the analysis, but may desire to know further the quantity, 4 of sulphur,—5 of copper,—6 of oxygen,—7 of hydrogen. As means of gaining this information, he naturally considers the quantity of various re-agents that may be employed for discovering the quantity of sulphuric acid.—8. How much barytes,—9 carbonate of barytes,—or 10 nitrate of barytes, would be requisite for this purpose. 11. How much lead is to be used in the form of 12 nitrate of lead; and when the precipitate of, 13, sulphate of barytes, or, 14, sulphate of lead, is obtained, it will be necessary that he should also know the proportion which either of them contains of dry sulphuric acid. He may also endeavour to ascertain the same point by means of 15, the quantity of pure potash, or 16, of carbonate of potash, requisite for the precipitation of the copper. He might also use 17, zinc, or 18, iron, for the same purpose, and he may wish to know the quantities of 19, sulphate of zinc, or 20, sulphate of iron, that will then remain in solution."

With the exception of the sixth and seventh, all these questions may be satisfactorily answered by bringing the number indicating the quantity under examination opposite crystallized sulphate of copper, with 5, water. Occasionally, however, the scale will not answer other questions, as, for instance, proportions below 10, in consequence of the series commencing at that number, but by doubling or using any convenient multiplier, as 10 or 100, the quantity of substance (as for example, those formed by the union of either hydrogen or carbon with some other element), and then halving or dividing the gross amount by 10 or 100, according to the multiplier which has been used, the problem is satisfactorily solved. Another imperfection which accompanies Wollaston's scale, and might lead to more or less confusion, ought to be guarded against. It is this. In consequence of most chemists having now pitched upon hydrogen as unity, and the atomic weight of oxygen as 8, the proportional numbers of Wollaston will require a little adjusting. That chemist fixed upon oxygen as his standard of comparison, and called it 10; it will, therefore, be seen, instantly, by those who have adopted hydrogen, that by a little arithmetical skill the relations of the

different numbers may be easily ascertained.

The value of this instrument not only to the scientific, but also to the manufacturing chemist, is exceedingly great. It was a most happy invention, as it directly answers a hundred questions which might occupy much time in arithmetical calculations to determine. Indeed so numerous are the advantages resulting from its use, that that student does himself great injustice who neglects to include it amongst his other apparatus.

Since the formation of Wollaston's, other scales have been introduced to the notice of men of science. Those of Brande and Reid are, perhaps, the best, inasmuch as those chemists have adopted the Daltonian equivalents, both of them fixing upon hydrogen as unity. For the use of medical students and junior chemists, they are decidedly preferable to Wollaston's, since it is not unlikely that hydrogen may be hereafter found to be the lightest of all bodies, in which case nothing can prevent the chemical world from adopting the opinions first promulgated by Dalton.

I shall next notice the second proposition.

PERMANENT CONTRACTION OF THE FINGERS.

To the Editor of THE LANCET.

IF the following observations should be considered sufficiently important, I shall feel obliged by their insertion in an early No. of THE LANCET, as a translation of Baron Dupuytren's clinical lectures on surgery has been published in that journal. I remain, very respectfully, &c.,

JOHN WINDSOR,
One of the Surgeons to the Manchester Eye Institution, &c.

Manchester, 5th month, 1834.

The history of surgical operations and inventions, by which human afflictions may be cured or alleviated, is a subject not only highly interesting in itself, as tending to show the gradual improvements in our art, but also satisfactory in ascribing to different persons their respective merits in co-operating towards its perfection.

I am induced to make these observations, by having read over the first of Baron Dupuytren's clinical lectures on surgery, on permanent retraction of the fingers, and, again, the recapitulating remarks at the completion of the course.

In the latter occur the following words: "Nous avons debuté par une circonstance heureuse, par un mémoire sur un sujet tout neuf; la rétraction permanente des doigts est une affection dont la cause avait été jusqu'alors inconnue, et la nature au-dessus des ressources de l'art: l'étude de l'anatomie pathologique a révelé à M. Dupuytren un traitement efficace. On a cherché dernièrement à ravir cette découverte à notre professeur, en l'attribuant à Sir A. Cooper," &c.

The following extract from my notes of the anatomical lectures, delivered by Astley Cooper and Henry Cline at *St. Thomas's Hospital*, will show that the merit of having *first* demonstrated the true knowledge of this affection, is not at least to be attributed to the justly celebrated French surgeon. The notes were made from a lecture by that ingenious and excellent surgeon, the late Henry Cline, jun., on the 8th of November, 1808, many years, as far as I can gather from the Baron's lecture (p. 2, No. 24), before he became informed of its true nature.

"The aponeurosis palmaris is found just under the skin and fat of the hand, and, like the aponeurosis pedis, it defends from pressure; it is attached at its upper part to the ligamentum annulare carpi; it is narrow at its origin, then spreads itself out amongst the cellular membrane of the palm, and dips amongst the muscles; it has several cross bands, tying, as it were, the processes together. Slitting into tendinous columns, it is inserted into the thecæ of the fingers, and bases of the phalanges.

"One or more of these tendinous columns of the aponeurosis palmaris sometimes becomes contracted and thickened; most generally one only is affected, but sometimes more, and proportionably so many fingers are bent into the palm of the hand. The *treatment* is easy and efficacious; it consists in cutting through the aponeurosis with a common knife. In performing the operation, carefully dissect through, fibre by fibre, the aponeurosis palmaris, in order to avoid the blood-vessels and nerves underneath; the finger or fingers may be kept extended afterwards by a splint, for the flexor muscle has in some degree become shortened, and without this the disease might be reproduced.

"It may be *distinguished* from a contracted state of the flexor muscles, by *all* the fingers being then generally drawn into the hand, or if one only, there is not the same starting into the palm of the hand when the finger is stretched, as there is in a contracted state of the tendinous processes of the aponeurosis palmaris; the latter feels like a very hard cord raising the skin, but the flexors are too low to start thus, and are also

bound down by the ligamentum annulare."

Having no motive but the cause of truth in penning the above, I hope it will not be ascribed to any invidious feeling, as I entertain no feeling towards the Baron Dupuytren but that of admiration and respect for his talents and character, a character requiring no borrowed merits or misrepresentation for its rise and support.

EXPERIMENTS ON THE BLOOD.

By W. Reid Clanny, *M.D., Sunderland.*

I have been for some time impressed with the idea, that further inquiries and experiments were requisite in order to be enabled to understand the extent of the changes which take place when venal or arterial blood is submitted to the action of atmospherical air in the process of respiration.

In continuation of my experiments which are recorded in the pages of The Lancet, 172 and 173, Vol. I, for the year 1832-3, and pages 71 and 72, Vol. II, for the same year, I devised and performed the following:—Upon the 10th of last month twenty-three ounces of blood were taken from the arm of a sailor aged 60, who came to our infirmary for that purpose, as he found great uneasiness from fulness of blood in the head. Two ounces of this blood were received in vacuo in one of my twenty-ounce capped flasks, at mid-day, from which I removed the free carbon in the manner stated in the pages of The Lancet as above mentioned, and I found that the quantity thereof was 32¼ grains, having exactly the same tint as arterial blood.

I lost no time in extracting the atmospherical air from the flask; and turning the stopcock I inverted the flask, and immersed it in a column of spring water.

May 11, noon. Upon removing the flask containing the blood from the column of water, I observed that the blood had again assumed a venal tint. I now restored the flask to its proper position, and upon turning the stop-cock found that the vacuum had continued perfect, and that the atmospherical air rushed into the flask with considerable velocity. I recommenced the process for estimating the weight of the free carbon which might be found in the blood, during the last twenty-four hours, in its isolated state, and continued this process till the blood assumed the arterial tint, which occupied me thirty-five minutes. I now placed the flask, with proper precautions, in the scale, which had been kept in the same state for the last twenty-four hours, and found, that by the process the blood was exactly twenty-eight grains and a half less in weight than yesterday. The atmospherical air was again most carefully removed by the agency of the air-pump, and the flask replaced in the same column of water as recorded above.

12, noon. The isolated blood was even darker in tint than yesterday; with similar precautions, and by the same experiments as before, twenty-six grains and a quarter of free carbon were removed, and the blood again became arterial in its tint. After these experiments, the same plan was again pursued, and the blood was restored to the column of water in the pneumatic trough.

13, noon. The blood assumed to-day a rich claret tint, and in its texture and appearances reminded me of blood taken from the arm of a person in the last stage of typhus fever. Repeating my experiments, eighteen grains and a half were removed; and the flask with the blood was replaced in the column of water as before.

14, noon. I find that the blood has the same tint as yesterday, and that only eight grains and three quarters were removed by the agency of atmospherical air and the air-pump. The blood was replaced as before.

15, noon. This day I could remove only five grains and a half of free carbon.

16, noon. The blood now shows a dark claret tint, and I could remove only three and a half grains of free carbon.

17, noon. I find that only two grains are removed by the process.

18, noon. After long and anxious efforts, I was not enabled to remove the tenth part of a grain of free carbon from the blood to-day. Thus in eight days I removed from two ounces of venal blood 125½ grains of free carbon.

Upon the 3rd of this month (June) Henry Foster, aged 40, a dispensary patient, under my medical treatment for cephalalgia, was ordered to have the temporal artery opened, and twenty-two ounces of blood to be removed from it. This was carried into effect by Mr. Penman, house-surgeon to the infirmary and dispensary, in the presence of J. D. Mackenzie, M.D., a friend of mine from Edinburgh.

Two ounces of this arterial blood were submitted to investigation in the manner detailed above, and four grains of carbon were instantly removed. The atmospherical air was carefully extracted from the flask containing the blood, and the flask (with its contents) was placed for twenty-

four hours in a column of water in a reversed position.

June 4, noon. The blood had become dark in tint, and by the same process exactly three grains and a half of free carbon were removed. The blood was now isolated in a column of water as above recorded.

5, noon. To-day the alteration of tint was not so remarkable as yesterday, and though I worked the air-pump for half an hour, changing the atmospherical air from time to time, I could not thereby effect the least alteration in the weight of the blood.

These facts are so striking, that I invite the faculty to prosecute the inquiry, which being identified with animal vitality, and also with the phenomena of several acute diseases, such as typhus, hyperanthraxis, and asphyxia, demand our serious consideration. The plan is so simple, that any young medical man may follow up the inquiry at his leisure hours.

The candid reader will readily excuse me for referring to my "Lecture on Typhus Fever," which was published on the 1st of January, 1828.

Professor Berzelius, at page 80, vol. 7th of his "Traité de Chimie," French translation by M. Esslinger, Paris, 1833, makes mention of the results of my experiments upon the blood of typhus patients, to the following effect, which, for obvious reasons, I translate into English:—"Reid Clanny maintains that in nervous (i. e. typhus) fever, when the disease increases, the blood always contains more water, and, on the contrary, the water diminishes gradually in quantity as the disease leaves the patient." Had this distinguished philosopher attended to the context of my work, he would have discovered that though my experiments give the results he states, still he will find that this is only relative, and that I am compelled, not only by chemical experiments but also by facts and observations, to come to the conclusion that the proximate cause (as the disease itself) is neither more nor less than "a total cessation of sanguification."

When I am corrected in any subject in which chemical experiments have been employed, I feel obliged, provided I be not treated with undue severity; but when experiments and facts are refused, in order to make room for theories which are not supported by experiments, I hope I shall be pardoned if I express myself freely and fearlessly.

In Dr. Craigie's clinical reports, in the *Edinburgh Medical and Surgical Journal* for last April, I find the following paragraph:—"After the foregoing explanation of the mode in which the blood loses, during fever, its characteristic properties of spontaneous separation into clot, and serum, and fluidity, and is converted into a viscid semifluid dark-coloured homogeneous substance, I think it superfluous to advert particularly to those hypotheses in which the blood is represented to be deprived of the whole or part of its saline impregnation, as maintained by Dr. Stevens, or to have the proportion of its animal and saline principles diminished, as taught by Dr. Reid Clanny. Without pretending to deny entirely the facts thus represented, I conceive they are the effect of the disorder and atony of the general and pulmonary capillary systems during fever, and, though the cause of some of the symptoms of the disease, are not the cause of the disease itself."

Having been educated in the Cullenian school, I have hitherto taken for granted that the "proximate" cause and the disease itself, when fully formed, are synonymous, and when this is corroborated by such authorities as Dr. Parr, Mr. Lawrence, and other eminent medical men, I see no reason for changing those views which were taught me at college.

I am aware that the faculty feel anxious to discover the nature of the first impulse which the system receives when attacked by any disease, however hidden or mysterious it may be, but this is no argument for perverting the meaning of the phrase "proximate cause," nor can it be so.

I am not particularly satisfied with the word "entirely" as used in the paragraph by Dr. Craigie, just quoted, who, in his theory of typhus fever, substitutes in the same page vague conjectures for chemical or pathological investigations. This may satisfy himself, but it will not satisfy those who defer theorizing till they have proofs which cannot be set aside except by well-conducted experiments.

I acknowledge that the construction of the concluding paragraph just quoted is so obscure, that some doubt remains in my mind how it should be interpreted. If the author intend that the word "facts" shold be understood to stand for results of experiments, I can see what he wishes to express, and if so, I protest against the inference he draws as regards my views, for I have distinctly explained my impression to be, that in all cases of genuine typhus fever, the severity of the symptoms and morbid changes of the blood are progressive till the turn of the disease, if such occur, and if not, till the death of the patient; and that at the commencement

of the disease the blood is very little changed in its nature, inferring thereby that some obscure cause not heretofore discovered, produces in a gradual manner all the effects which I have enumerated in my lecture.

What shall we say to the pretended discoveries of Dr. Craigie, after reading the following extracts from my lecture? Page 18, "At the commencement of each paroxysm in typhus fever, such is the exhausted state of the system, that respiration does not convert the carbon and oxygen in the lungs into carbonic acid gas, in a sufficiently rapid manner; the extrication of that heat which ought to be (by the circulation of the blood) conveyed to all parts of the body. From exhaustion of the nervous power, the respiration becomes more languid, the whole system experiences a deadly coldness, and, as in all cases, when the blood circulates more slow, the capillary branches propel their contents with the greatest difficulty; and hence the gratuitous term, 'spasm or debility of the extreme vessels.'"

Page 13. "During the fatal progress of the disease, carbonic acid gas is not to be found in the blood, and except a turn take place by which fresh chyle is carried to the thoracic duct, the blood is rendered vapid, and, in some cases, it passes to a putrid state."

"As we cannot explain the modus operandi of the brain in the *process* of chylification, neither can we explain the manner in which it is impaired or suspended." —Page 16.

When Dr. Craigie shall show me experiments upon the blood of patients affected with typhus fever, and thereby prove to me, or to more competent judges, that I have been led into error, I will attend to him, and endeavour to profit thereby, but in the interim I hold myself excused from paying further attention to any of his conjectures.

My next paper will also be upon the interesting subject of human blood.

Sunderland, 12th June, 1834.

London University Hospital.—Arrangements have been made for its immediate completion, in order that it may be opened during the present year. The fee for medical and surgical practice during one year is to be 21*l.*, and during six months 15*l.*

THE LANCET.

London, Saturday, June 28, 1834.

In The Lancet of last week we inquired whether any Correspondent could favour us with a copy of the document wherein the objections of the surgeons of the *Birmingham Hospital* had been made with respect to the *Memorial* which was presented to the Parliamentary Medical Committee by the surgeons of the *Bristol Infirmary*. Mr. Jukes, one of the authors of the paper, has had the politeness to send us one for inspection. We took the liberty of terming the article an "enlightened" one, from an examination of two or three extracts which had been taken from it and printed in other Journals. Mr. Jukes, we apprehend, is annoyed at the expression, but we believe that a perusal of the paper will satisfy the mind of every impartial and intelligent person that it deserved to be designated thus ironically:

A Rough Draft of an intended Memorial from the Surgeons of the Bristol Hospital, praying the Legislature to make such alterations in the Charter of the Royal College of Surgeons in London, as will in future dispense with Residence in the Metropolis, having been presented to the Surgeons of the Birmingham Hospital for their approval:—

"They beg to state, that having carefully and maturely weighed all the objections stated therein, they respectfully decline taking any part in such petition.

"They do not consider the reasons of such moment or weight as to justify the taking away from the Court of Examiners of the Royal College of Surgeons such a power as is complained of, viz. the compelling the six months' attendance at a London Hospital.

"They consider Provincial Medical Schools of great value to the Students of the Medical Profession, as elementary ones, and as laying the ground-work of sound anatomical knowledge in the early career of the student, and they think the Court of Examiners of the Royal College of Surgeons in London, have with sufficient

liberality met the views of such preparatory schools in remitting attendance on the Metropolitan hospitals to the above-named period.

"In reply to the objections, viz. *in the first place*—'*The opportunities being fully as adequate in most of the Provincial Hospitals for every purpose of education as those in the Metropolis*'—

"We beg to observe, that we see great advantage resulting, and always likely to result from Students being obliged to attend for six months at the Metropolitan Hospitals. The comparisons they will necessarily draw from the diversity of practice in operations, treatment, &c., will be of infinite use to them when they themselves become hereafter active and responsible practitioners.

The second objection—"That the morals of Students are more likely to become corrupted in London than in the larger provincial towns"—*is not at all borne out by facts.*

The third objection—"That owing to the present existing restrictions, Students very commonly neglect to avail themselves of present advantages, in the hope that they shall have full opportunity of redeeming them when in town"—is, in our opinion, *not valid.* The idle and dissolute are to be found in every walk of life, and with the existing privileges we do not believe that Students in general are so negligent of their own future interests as the objection would seem to imply.

The fourth objection—"That it implies an invidious distinction between the Teachers in the Metropolis and those in the Provinces"—is, in our opinion, wholly groundless.

The fifth and last objection—"That it occasions a very considerable increase in the pecuniary expenditure, without any advantages in any degree correspondent to the extent of the outlay"—must be considered *a mere matter of opinion*, and subject to various considerations.

"We consider the privileges already granted to Provincial Schools, so far as the Apothecaries' Company is concerned, very injurious, and likely to be still more so to the Profession in general, owing to the numbers of Students who merely pass their examinations at Apothecaries' Hall, and which they can do without residence in London, or attending any Surgical Practice in the Provincial Hospitals, and who immediately begin as General Practitioners in Medicine and Surgery, in violation of the interests of the Members of the Royal College of Surgeons.

RICHARD WOOD, JOSEPH HODGSON,
BOWYER VAUX, ALFRED JUKES."

This is altogether an exceedingly foolish production, and the surgeons of the Birmingham hospital have undertaken to make a laboured commentary on subjects which it is perfectly evident they do not in the least degree understand. On the *law* of the case their ignorance is most amusing. In the first place, they do not consider the reasons which were advanced by the Bristol surgeons, to be of sufficient force to justify the legislature in taking from the Court of Examiners the power of compelling a six months' attendance at a London hospital. Be it observed, that down to the end of the third paragraph no mention is made of *medical students*, although reference is made to a "dispensation of residence in the metropolis, and of attendance at a hospital." What excuse can educated men advance for writing thus loosely and incorrectly?

If the incorrectness, however, of the style in which the article is written is highly objectionable, the evidence of ignorance on the subject of medical government is actually offensive. Are these gentlemen aware that the authorities of the College of Surgeons can exercise no such power as that of compelling a student to attend six months at a London hospital? The law has given to that College no power over the management of the hospitals in any way; the Governors of those institutions might exclude every student in the metropolis from observing the treatment of a single patient belonging to the wards. It is not the *attendance* which the College has insisted on so pertinaciously, but on the production of a "*certificate*," implying that a certain sum of money has been paid to themselves, their relatives, or their friends, as the surgeons of those hospitals. This is the secret of their government—the key of their system.

The laws of the College are framed to suit the purposes of the parties connected

with the College, the public and the mass of the profession being always left out of consideration, unless at those times when calculations are made as to the extent of plunder to which people will submit without complaint.

Power, then, in law, the College has none over the hospitals, and the students, therefore,—when they *do* attend—make their visits under the permissive sanction of the Governors.

Why, we would inquire, should students be compelled to pay any money whatever to the officers of the *London* institutions, if they are enabled to acquire a competent knowledge of their profession in the country? An examination, fully capable of testing their ability, could be instituted, and when they are capable of undergoing an efficient scrutiny, the right of practising ought then to be conferred by the authorized administrators of the law. It is a fact with which the surgeons of the Birmingham Hospital must be familiar—although, for reasons with which we are unacquainted, they make no mention of the circumstance—that eight out of every ten students who pay the fees for attendance at those hospitals of the metropolis which are crowded with pupils, receive, in return, merely "tickets" and "certificates," the practice of those hospitals being conducted in such a manner, that it is only those parties who are in some way connected with the officers, in the character of dressers or clinical clerks, by whom the least useful information can be acquired. The testimony of every experienced man is, in short, in favour of the superior advantages which the smaller hospitals of the country are known to afford. *There*, sufficient time is allowed, in the daily visits, for the examination of the cases; the student, free from bustle and confusion, can observe, uninterruptedly, every stage of the treatment, and has sufficient leisure afforded to him for exercising his judgment upon the various objects which are presented to the influence of his senses.

The remarks which are appended to the second, third, fourth, and fifth objections, are rendered so thoroughly ridiculous, from the disregard which is manifested in them to truth and fact, that they are undeserving any further notice. In short, the entire document is a piece of nonsensical twaddle, and is precisely such a production as we should have expected would emanate from the battish officers of the Birmingham institution in favour of their battish contemporaries attached to the monopolizing institutions of the metropolis.

In another part of our journal will be found a report of the proceedings of a meeting of the Weekly Board of St. George's Hospital. The members of the Board, it should be observed, are the superintendents of the household proceedings of the establishment; they are authorized to call meetings, on receiving a requisition from five or more governors, to adopt final measures with regard to all trivial matters, and preliminary measures with regard to all important matters.

The report, to which we have already adverted, exposes the first attempt towards accomplishing one of those jobs which have rendered the hospitals of this metropolis so disagreeably notorious. An intimation having been issued, that an office for a *second* assistant surgeon was to be created—the *first*, be it observed, having declared that he had little or nothing to do—three candidates at once started into the field. One is the *bonâ fide* claimant; the other two may be considered as *shams*, brought forward for the purpose of turning what was really a farce into a serious drama. The two supernumerary actors are Mr. Palmer and Mr. Lane.

If the governors become acquainted in sufficient time with the fraud which it is intended to perpetrate in this school of the "Eminents," we believe that it will be frustrated, and that the chief performers in the nefarious scheme will be exposed and defeated. A faithful report of the proceedings of the next meeting shall appear in our pages.

Before leaving this subject we should state, that, in accordance with the secret method which Mr. BRODIE has employed to carry this job into execution, Mr. CUTLER is now prosecuting an active canvass, which he commenced *before* the meeting of the weekly Board, and in furtherance of his views is circulating among the Governors a "certificate" of his competency—which we also publish—procured from the obliging hands of Sir ASTLEY COOPER.

DUBLIN COLLEGE OF SURGEONS.

(From our private Correspondent.)

Mr. KIRBY's evidence before the medical committee of the House of Commons will, we have no doubt, be extremely valuable; it will, if we mistake not—*si vite remetior astra*—very satisfactorily frustrate the objects for which its author has been appointed to advocate the case of the Dublin College of Surgeons in London. Let us see how far we are warranted in this anticipation.

Mr. KIRBY commenced his career as a private teacher of anatomy, and as a radical reformer in medical politics. For a period of upwards of twenty years he was the sworn enemy of the abuses and illiberality of every corporate institution, but more particularly of the Royal College of Surgeons in Ireland, in the councils of which body he maintained the principles of *free trade* in the tuition and practice of the medical sciences, while the theatre of Peter-Street School annually resounded with the loftiest and bitterest denunciations of the selfish and circumscribed policy of his chartered opponents. On all those occasions of personal and pompous complaint, he put himself forward as the victim of collegiate jealousies and intrigues, at the same time ingeniously representing himself as an object of popular sympathy and support. In that species of bombastic eloquence which flows from the head, not the heart, and in which Mr. KIRBY was a practised adept, it was insinuated, or broadly asserted, to the students of the Irish metropolis, that his fate and theirs were the same, and that some scheme was always in progress at the College for their mutual destruction. The lure was effective, and the crowded benches of the Peter-Street School testified for many years how readily our feelings may be aroused in favour of the persecuted and oppressed.

During this long and well-supported scene of deception, the actors were, to a man, with the exception of Mr. KIRBY himself, perfectly sincere in the parts which they sustained! The College authorities were, no doubt, as criminal in their hostile intentions towards Mr. KIRBY, as he represented, and as the medical public and students of Dublin believed them to be, on his showing. He alone was the MOKANNA of the drama, for Mr. KIRBY is now a professor of the College of Surgeons in Ireland, and one of the most active supporters of its abuses! It is possible, no doubt, that a man may, by honesty, ability, and continued opposition to corruption, force his way into the professorship or presidency of a corporation, without any abandonment of his principles, or debasement of his nature. Such a phenomenon we conceive to be with in the scope of possibility; but is it exemplified in Mr. KIRBY's case? The first step in Mr. KIRBY's promotion (degradation we should say) settles the question for ever. Covetous of the profits of Peter-Street School, and desirous of adding them to the joint-stock purse of the College, but yet afraid to attempt their appropriation by the enactment of a by-law professedly for that purpose, the professors of that institution came to a resolution of effecting their object by a more indirect but not less disreputable route. After mature deliberation, the plan fixed on was to in-

corporate Mr. KIRBY with the monopolists of the College, to compensate him for his *burking* of Peter Street, by giving him a place at the chartered board of the College. To carry this design into execution, an emissary was sent to him to accept of the professorship of the practice of physic in the College, vacant by the resignation of Dr. MARSH. Mr. JACOB was the fitting herald employed in the vile servitude of corruption on this occasion. Mr. KIRBY only wanted a gulf to throw himself into, and an appropriate one was opened to him in this extraordinary mission. The Circean cup was offered to him by the College Ganymede, but, before allowing its contents to cross his lips, he stipulated, in the genuine spirit of corporate debauchery, that before the moral of the fable was consummated in his conversion into a lover of the mire, his election would be secured him against all opposition.

Nor was this a matter so readily managed; but what will not turpitude and audacity accomplish when united? Amongst the old by-laws, there was one which opened the professorship to all candidates having a degree from one of the universities; the general applicability of this law was therefore to be circumscribed, in order to exclude Mr. KIRBY as much as possible from competition. A by-law was accordingly enacted, requiring the candidate to have a surgical diploma from one of the colleges, and to be, moreover, educated according to the schedule laid down by the Dublin College of Surgeons. It was well known that there was no candidate who had this surgical diploma; and if there were, his case would have been very easily disposed of by the ambiguous wording of the second clause in the ordinance specially enacted for Mr. KIRBY'S appointment. We must not proceed further in our exposition of this scandalous job. Mr. KIRBY was elected in consequence. But by whom? Why, by the very men who are sworn on oath to hold no communication whatever with candidates for professorships in the College of Surgeons! Yet these are the very men who proposed, who invited, Mr. KIRBY to take office in the College, and who enacted a by-law specially to secure the perpetration of their perjury. And these, forsooth, are the individuals whose *parole* evidence is to be believed in the House of Commons! If it be, why, then, let it,—with all our hearts.

But this was not all; Mr KIRBY was as generous in his fall as his new associates were profuse in their corruption. Having driven the best bargain he could for himself, he next stipulated for the appointment of his son-in-law, Mr. WEST, to a demonstratorship in the College, a situation similar to that which he held under Mr. KIRBY in Peter-Street, and Mr. WEST was accordingly nominated to the office in Stevens' Green! These are not conjectures or opinions of ours. For the truth of what we have here stated, we refer to Mr. KIRBY's printed introductory lectures; to the by-laws of the Colleg[illegible] and to Mr. JACOB, who cannot de[illegible] part we have imputed to him in th[illegible] action. We could readily [illegible] stances of a similar tenden[illegible] presume what we have ju[illegible] more than sufficient to stamp[illegible] of Mr. KIRBY's evidence be[illegible] mittee of the House of C[illegible] have, in the case before us, t[illegible] radical for twenty years of [illegible] once changing his opinions, [illegible] of opinion being accompanied [illegible] stances which exclude the poss[illegible] our believing that it was the resu[illegible] pure and virtuous exercise of reason.

Of what value then, we ask, can be the evidence of such an individual on a question in which prevarication and concealment will be matters of feeling, of interest, nay, we had almost said of necessity? Yes, that evidence, we again repeat it, will be valuable as showing the stuff of which the College School and its miserable dependants and expectants are composed. We trust, therefore, that no accident will deprive the cause of truth and justice of the valuable assistance of JOHN TIMOTHY KIRBY'S evidence before the Medical Committee of the House of Commons. We once heard of an Irishman, who was about to indict one of his neighbours for an assault, asking a magistrate for the "strongest warrant his honour could give him," as the character of the man for whom it was intended was none of the

best. We would be for making a similar request to Mr. WARBURTON, and expect that he will dispatch one of his strongest summonses for Mr. KIRBY, for we could not afford the loss of his excellent testimony to the corruption of the College.

Besides Mr. KIRBY, there are two other witnesses to be examined on the part of the College, Mr. REID and Mr. TAGART. Of these gentlemen it is unnecessary for us to speak; they may advance erroneous opinions, but we have no doubt that their evidence will be honest and sincere. Upon this point the College would seem to have some misgiving: they have discovered (but the discovery came too late), that Mr. TAGART is not precisely the man they ought to have selected for the occasion.

Mr. NUNN.—While on the affairs of the [illegible] College, we take the opportunity [illegible] this gentleman whether he has [illegible] to the hands of the com- [illegible] *not*. Some persons do [illegible] they are *spit-on*. 'Tis [illegible] the tyrant. Will Mr. [illegible] er kick?

[illegible] ED LETTER.

[illegible] AL COMMITTEE.]

[illegible] MACMICHAEL,—Though, [illegible] rtant points respecting our [illegible] ns before the parliamentary [illegible] ittee I have written to Dr. SEYMOUR, there are still some which I can only confide to *you*. SEYMOUR has every inclination to make himself a busy body, and I cannot always venture to confide in his discretion. He is a sharp fellow in his way, and has all the flexibility of his father, who I recollect so well as the vestry clerk of Marylebone, yet he talks too much, and I am confident that he often injures our cause. A man who talks much should, like myself, have a clear conscience.

"What I wish you, my dear friend, to bear in mind is, that the whole strength of our case depends on defending openly and roundly—and in unmeasured terms—the superiority of an Oxford and Cambridge education above that of all the other universities in the empire, for two things,—MORAL discipline and CLASSICAL learning. As to the *moral* conduct, I do not wish my name dragged before the public on that point. Those who know me best know, and have always duly appreciated, my virtues; but evil-disposed people, of whom there are many in these times, might make some impertinent allusions to my exceedingly delicate and trying Windsor duties during the reign of GEORGE the FOURTH, my beloved monarch and personal friend, in conjunction with Sir WILLIAM KNIGHTON and the MARCHIONESS, and also to my morning visits at Kensington Palace.

"What I require of you to do is, to have in readiness proofs, undeniable proofs, of the literary accomplishments and classical learning of the Fellows, and of their important contributions to medical science. I do not see how you could exemplify their classical knowledge better, than by a reference to the 'oration' I delivered a few weeks ago at the College, 'on the duties and qualifications of a physician.' Indeed, as you well know, I chose that subject, and took that opportunity, in order that I might display to the public the talents and acquirements of a Fellow of the College, as a physician. You will observe that I have made in that short oration no less than ten quotations from the ancient Roman authorities. But it is not in the number so much as their aptness, and the classical taste shown in selecting them, that their great merit consists, and I am confident they cannot, if well displayed, fail to make a deep impression on the Committee. You could not do better, perhaps, than quote the following paragraph in that oration, wherein I think our high moral feeling and delicate sentiment are well shown, the composition, too, being in *pure* English, and such as it could not be expected that any man should write who had not been educated either at Oxford or Cambridge.

"'It was a favourite remark,' I observe in that oration, 'of a very accomplished Roman—Pomponius Atticus—that "sui cuique mores conciliant fortunam;" and the motto of William of Wykeham, to whom many of you have been indebted for your education, was, "Manners maketh man."

"'Now we shall do great injustice, both to the Russian senator and to the illustrious ecclesiastic of our own country, if we interpret the "mores" of the former and the "manners" of the latter by the mere personal demeanor of a man. They must be construed into his principles, his generous sentiments; what is in a soldier, his honour; in a lawyer, his integrity; in the churchman, his exemplary *carriage and conduct*; and in a physician, all that is enjoined in the oath of Hippocrates; not only a consummate knowledge of the resources of his art, but a

corporate Mr. KIRBY with the monopolists of the College, to compensate him for his *burking* of Peter Street, by giving him a place at the chartered board of the College. To carry this design into execution, an emissary was sent to him to accept of the professorship of the practice of physic in the College, vacant by the resignation of Dr. MARSH. Mr. JACOB was the fitting herald employed in the vile servitude of corruption on this occasion. Mr. KIRBY only wanted a gulf to throw himself into, and an appropriate one was opened to him in this extraordinary mission. The Circean cup was offered to him by the College Ganymede, but, before allowing its contents to cross his lips, he stipulated, in the genuine spirit of corporate debauchery, that before the moral of the fable was consummated in his conversion into a lover of the mire, his election would be secured him against all opposition.

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Mr. NUNN.—While on the affairs of the Dublin College, we take the opportunity of asking this gentleman whether he has put his case into the hands of the committee. *We fear not.* Some persons do not know when they are *spit-on.* 'Tis the slave that makes the tyrant. Will Mr. NUNN wait for another kick?

INTERCEPTED LETTER.

[THE MEDICAL COMMITTEE.]

"DEAR DR. MACMICHAEL,—Though, on many important points respecting our examinations before the parliamentary committee I have written to Dr. SEYMOUR, there are still some which I can only confide to *you*. SEYMOUR has every inclination to make himself a busy body, and I cannot always venture to confide in his discretion. He is a sharp fellow in his way, and has all the flexibility of his father, who I recollect so well as the vestry clerk of Marylebone, yet he talks too much, and I am confident that he often injures our cause. A man who talks much should, like myself, have a clear conscience.

"What I wish you, my dear friend, to bear in mind is, that the whole strength of our case depends on defending openly and roundly—and in unmeasured terms—the superiority of an Oxford and Cambridge education above that of all the other universities in the empire, for two things,—MORAL discipline and CLASSICAL learning. As to the *moral* conduct, I do not wish my name dragged before the public on that point. Those who know me best know, and have always duly appreciated, my virtues; but evil-disposed people, of whom there are many in these times, might make some impertinent allusions to my exceedingly delicate and trying Windsor duties during the reign of GEORGE the FOURTH, my beloved monarch and personal friend, in conjunction with Sir WILLIAM KNIGHTON and the MARCHIONESS, and also to my morning visits at Kensington Palace.

"What I require of you to do is, to have in readiness proofs, undeniable proofs, of the literary accomplishments and classical learning of the Fellows, and of their important contributions to medical science. I do not see how you could exemplify their classical knowledge better, than by a reference to the 'oration' I delivered a few weeks ago at the College, 'on the duties and qualifications of a physician.' Indeed, as you well know, I chose that subject, and took that opportunity, in order that I might display to the public the talents and acquirements of a Fellow of the College, as a physician. You will observe that I have made in that short oration no less than ten quotations from the ancient Roman authorities. But it is not in the number so much as their aptness, and the classical taste shown in selecting them, that their great merit consists, and I am confident they cannot, if well displayed, fail to make a deep impression on the Committee. You could not do better, perhaps, than quote the following paragraph in that oration, wherein I think our high moral feeling and delicate sentiment are well shown, the composition, too, being in *pure* English, and such as it could not be expected that any man should write who had not been educated either at Oxford or Cambridge.

"'It was a favourite remark,' I observe in that oration, 'of a very accomplished Roman—Pomponius Atticus—that "sui cuique mores conciliant fortunam;" and the motto of William of Wykeham, to whom many of you have been indebted for your education, was, "Manners maketh man."'

"'Now we shall do great injustice, both to the Russian senator and to the illustrious ecclesiastic of our own country, if we interpret the "mores" of the former and the "manners" of the latter by the mere personal demeanor of a man. They must be construed into his principles, his generous sentiments; what is in a soldier, his honour; in a lawyer, his integrity; in the churchman, his exemplary *carriage and conduct*; and in a physician, all that is enjoined in the oath of Hippocrates; not only a consummate knowledge of the resources of his art, but a

gentleness of manner; a sacred reserve *as to the affairs of families* into which he may be admitted; a delicacy and a *chastity* proof against all temptation. In short, he must not have witnessed sacrifices to Moloch, or the rites of Flora, "ubi Cato spectator esse non potuit."

"'But manners, in this sense of the word, are the result of education. Uneducated man knows nothing of sentiment. He is governed by two predominant and paramount objects—the gratification of his passions, and the appropriation to himself of everything to which he may take a fancy. Education, conducted upon Christian principles, eradicates this selfishness gradually, and finally makes him fit for society. He is taught to see the propriety, as well as the immediate advantages, of reciprocal kindness; of conceding something which he possesses to the wants of others, and of receiving in return similar accommodation. Presently he anticipates the wishes of his companion, and volunteers the gratification of them, and thus lays the foundation of a friendship in the "idem velle atque idem nolle." At length, by good examples, he acquires the essential principles of good breeding, "nunquam se præponens aliis, adversus nemini;" and as far as proper feelings are concerned, which are best obtained and improved by communication and close intercourse with those who possess them, he is so far prepared to fulfil his duties in society.'

"Neither do I see any impropriety in your reading to the Committee the observations made by Dr. FERGUSSON in his review of my important work in the *Quarterly*, and which I deemed it expedient that my publisher should add to the advertisement, for really in these times a man is in duty bound to talk well of himself, as everything is now done to depress great people:—

"'The President of the Royal College of Physicians' (that means me) 'the President of the Royal College of Physicians, says the reviewer, 'has produced, in these Essays, *a delightful compound* of professional knowledge and literary taste. *Handled* with skill and feeling *such as his*, subjects of medical research have not only *nothing dry* and repulsive about them, but are of deep and universal interest and attraction. His points of view and illustrations are, in general, those of *a man of the world*, as familiar with *men and women* as with *books*; his language is that of a *graceful scholar*, and the reflections interspersed are not *more* remarkable for sagacity than agreeable for the benevolence and humane spirit which they reflect.'

"I wish, by-the-by, this last sentence had conveyed a less equivocal compliment. FERGUSSON should have said 'are *very* remarkable,' instead of 'not more.' But unquestionably he meant no quiz.

"As I anticipate that you will be interrogated sharply as to the scientific contributions of the Fellows, and the title they have to the high consideration which they claim, you must be prepared with ready answers, and know all their works. After having noticed my 'Essays and Orations,' as perhaps the most valuable present which has been made to medical science in modern times, you may next, with great propriety and truth, mention your own 'Gold-Headed Cane.' That book is an excellent proof of the high intellectual discipline and classical education which you yourself underwent at Oxford, and your dedication of the elegant little work to Lady HALFORD, was a proof of your good taste and high moral feeling. The volume, indeed, is in every respect highly creditable to an accomplished physician. The works of Drs. SEYMOUR, LATHAM, and PARIS, have all contributed greatly to the advancement of science.

"Our opponents will very probably state to the Committee, as a proof of the alleged impotency of the Fellows in the paths of the science, that their contributions have been very meagre, and that during the last 68 years the College have published only six volumes of 'Transactions.' You can have no difficulty in stating that it is not necessary to write great works now-a-days. The science is so much advanced, that it is not to be expected that we should have any voluminous publications.

"The questions which may be asked respecting our medical education at Oxford and Cambridge must be maturely considered, and may lead to some unpleasant exposures. I would wish you, therefore, particularly to avoid mentioning the circumstance that there happen to be at this moment three medical professors at Cambridge who are in holy orders,—the Professors of Anatomy, Chemistry, and Botany, all clergymen; and though the Downing Professor, my esteemed friend, Dr. HEWIT, has not been in the habit of delivering lectures on the practice of physic, yet he is not in holy orders, and he has always conscientiously given *notice* of the lectures, though he has very properly declined to deliver them unless at least six students entered to them.

I must say adieu suddenly. The Duke of GLOUCESTER is just taken very ill, and

has brought up seven quarts of white-bait. I must see his Royal Highness instantly.

"Yours ever,

——— ———.

"London, 1834.

"P. S. I have desired Dr. SEYMOUR to call at THE LANCET office as he passes with Dr. SHEE, to purchase for me three numbers of THE LANCET, which are missing out of my set. SEYMOUR I find a most useful man in doing this sort of job."

ST. GEORGE'S HOSPITAL.

THE NEW ASSISTANT-SURGEONCY.

Last Meeting of the Weekly Board.

ALL the medical officers of this hospital were present at this meeting, as also were a few of the lay governors who had got information of the intended job. After a few cursory observations,

Mr. BRODIE, with his usual simplicity and openness of manner, stated his intention of making an office for another assistant-surgeon, and he hoped that the Board would consent to his proposition, and recommend the same at a general meeting of the governors.

Mr. MORLEY then rose and said,—I beg to oppose the intended appointment. I disapprove of the principle on which it is intended to be filled up. I consider any additional appointment quite unnecessary; and I am informed that the one now proposed is only meant to further the personal views of Mr. Brodie towards his assistant in private practice, and *not* for the benefit of this charity. I will therefore oppose the measure. (*Cheers.*)

Mr. BRODIE replied nearly as follows:—It is improper that any lay governor should presume to give an opinion on this point (*groans*). I know this institution well, and what is *best* to be done for it. With respect to the gentleman I am about to propose, he is, in many respects, fit for all I intend him to perform, and it is suitable to me to have a *junior* at my elbow, who is accustomed to do all things just as I tell him. I don't want a man to supply my place.

Dr. SEYMOUR remarked, that it was presumption of Mr. Morley to offer any opposition to the plans of the medical officers of this hospital. They knew best the interests of the hospital as well as their own. (*A laugh.*) Considerable confusion here ensued, and several gentlemen endeavoured to speak at the same time, when

Sir CHARLES MANSFIELD rose and entreated that not more than six gentlemen would deliver their sentiments at a time.

Mr. WALKER, the assistant-surgeon of the hospital, then said—I consider it to be my bounden duty to oppose the contemplated appointment. I perfectly agree with Mr. Morley, in considering the *principle* of such an appointment quite disgraceful, and I contend that another assistant-surgeon is perfectly *unnecessary*. The duty of each of the four surgeons lies within a very narrow compass; and though I have been the only assistant for a considerable time, I have had scarcely anything to do. It therefore appears to me that the appointment of another assistant-surgeon would be a preposterous measure, and that even proposing such a thing to the governors would be highly discreditable. I have also a right to complain of the "hole-and-corner" mode of proceeding in this affair, as I was not even informed of Mr. Brodie's plan until yesterday evening.

Mr. BRODIE now brought forward a requisition to call a general meeting of governors, which he got signed by four of his governors (besides himself), one of whom, Mr. Gaskoin, has since declared that he was "kidnapped" on the occasion.

Mr. FULLER manfully objected to the abrupt manner in which this matter had been brought forward, and proposed that the general meeting should be called at some more distant period, and when such a change as proposed by Mr. Brodie had been maturely deliberated on.

Dr. WILSON also made some trite observations against the measure, and so did the renowned George Gregory.

Dr. RODERICK MACLEOD followed the footsteps of his fellow-labourer in manuring the green fungus, Dr. Seymour. (*Left debating.*)

NOTE FROM A GOVERNOR.

To the Editor of THE LANCET.

SIR,—I herewith enclose you a copy of a certificate from Sir Astley Cooper to Mr. Cutler, forwarded to me enclosed in an envelope with Sir Astley's name affixed on the corner. I leave you to judge of the propriety of such a procedure.

A GOVERNOR.

"It is with the utmost pleasure that I state my firm belief in the talents and acquirements of Mr. Edward Cutler, and my confidence in his ability to fill the situation which he is anxious to obtain with credit to himself and advantage to the Institution.

"ASTLEY COOPER.

"Conduit-street, June 13, 1834."

NOTE FROM ANOTHER GOVERNOR.

To the Editor of THE LANCET.

SIR,—I enclose you a copy of a letter from Mr. Walker, which will give you, and your numerous readers, some idea of what is going on at St. George's. I am your obedient servant,

A GOVERNOR.

"SIR,—As it is intended to propose the appointment of a second assistant-surgeon to St. George's Hospital, and feeling as I do, most conscientiously, that such additional officer is not in any way required for the benefit of the charity, I most earnestly beg your attendance at the hospital on Friday, the 27th inst., at 12 o'clock, the time appointed for considering this question, when I trust I shall be able to satisfy the governors, that such an appointment is not demanded by the quantity of labour, and will be in no way conducive to the real interests of the institution. I have the honour to be, Sir, your most obedient servant,

"B. B. WALKER,
Assistant-Surgeon to St. George's Hospital.

"2, Curzon Street, May Fair,
June 20th, 1831."

CORRESPONDENTS.

THE correspondent who adopts the signature of "*A Friend*," must, if he be in general a clear-sighted man, have read the note in question through some singularly mystified medium. We did not contradict any statements which had been made by Dr. Elliotson. Nothing could be further from our intention, in penning the notice in question, than to throw any imputation on the character or veracity of that gentleman.

Letters received from *M. B. O., C. G. B., A Pupil of St. George's, Mr. Jones, A disappointed Pupil*, and many other correspondents.

A Country Reader. No existing regulations would be evaded by it.

Mr. James's and many other papers next week.

NOTICE.—It is particularly requested that ALL letters of business forwarded to the Office of THE LANCET,—whether they relate to the contents of the Journal, or refer to transactions connected with the Commission Department,—be INVARIABLY addressed to THE EDITOR—Mr. WAKLEY. If this rule be not observed, it is impossible to guard against the impositions of anonymous scribblers who appear to have no other employment or pleasure than that of subjecting the conductors of journals to a heavy extortion in the shape of postage. If all letters of business transmitted to the office be addressed to the Editor, and no letters, unless so addressed, be received except the postage be paid, impositions will be effectually prevented.

ELECTION FOR FINSBURY.

TO THE MEMBERS OF THE MEDICAL PROFESSION, ELECTORS OF THE BOROUGH OF FINSBURY.

GENTLEMEN,—In consequence of your kind and extraordinary exertions in my favour, it is in my power to announce that I have nearly concluded one of the most successful canvasses ever witnessed. Earnestly entreating you, therefore, to be unremitting in your valuable exertions on my behalf, —or rather on behalf of the cause of MEDICAL REFORM,—I respectfully take this opportunity of informing you that the poll-booths will be open for recording the votes of the electors, at *nine* o'clock on Monday next.

The booths will be opened in the following situations:—

Lincoln's-inn-fields,
Queen-square,
Islington-green,
Spenser-street,
King-square.

Everything depends on prompt and early exertions. If there be hesitation or delay, all may be lost, because the poll, on the second day, may be closed *finally* before a sufficient number of voters are recorded to secure the triumph of the liberal cause. Let me beseech you, therefore, to continue your energetic labours, and induce your friends to fly to the poll at the earliest possible moment on Monday morning, as it is not improbable that the candidate who obtains the highest place at the conclusion of the first day's polling will maintain that position on the termination of the contest.

To you, gentlemen, it is unnecessary to say more. If the exertions of my medical friends continue to be maintained with that spirit which has already been exhibited during the progress of the election, triumphant and brilliant will be the success of

Your faithful friend,
THOMAS WAKLEY.

Greenford, Middlesex, June 26.